THE AMERICAN JOURNAL OF PSYCHOLOGY

EDITED BY
MARGARET F. WASHBURN, Vassar College
MADISON BENTLEY, Cornell University
KARL M. DALLENBACH, Cornell University
EDWIN G. BORING, Harvard University

WITH THE COÖPERATION OF
RAYMOND DODGE, Yale University; S. W. FERNBERGER, University of Pennsylvania; J. P. NAFE, Washington University; R. M. OGDEN, Cornell University; JOSEPH PETERSON, Peabody College; W. B. PILLSBURY, University of Michigan; A. T. POFFENBERGER, Columbia University; E. S. ROBINSON, Yale University; C. A. RUCKMICK, University of Iowa; G. M. STRATTON, University of California

Vol. XLV

MORRILL HALL, CORNELL UNIVERSITY
ITHACA, NEW YORK
1933

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

General
Bound October 1933

COPYRIGHT, 1933
BY
KARL M. DALLENBACH

49132

BF
1
E87
v.45

TABLE OF CONTENTS

Articles and Notes

ANDERSON, I., and CROSLAND, H. R., A Method of Measuring the Effect of Primacy of Report in the Range of Attention Experiment 701
ANDERSON, J. E., June Etta Downey: 1875-1932 362
ARONS, L., Serial Learning and Generalizing Abstraction 417
BARLOW, M. C., The Influence of Electric Shock in Mirror Tracing 478
BARRY, H. JR., and BOUSFIELD, W. A., The Visual Imagery of a Lightning Calculator 353
BARRY, H. JR., VERVEER, E. M., and BOUSFIELD, W. A., Change in Affectivity with Repetition 130
BECK, L. F., A Reset Dial for Chronoscopes 519
BECK, S. J., Configurational Tendencies in Rorschach Responses 433
BENNETT, G. K., and WARD, L. B., A Model of the Synthesis of Conditioned Reflexes 339
BENTLEY, M., Mind, Body, and Soul in Medical Psychology 577
BENTLEY, M., and VARON, E. J., An Accessory Study of "Phonetic Symbolism" 76
BERGMAN, L. V., and DALLENBACH, K. M., Portraits Useful to the Psychologist 165
BODER, D. P., Some New Electronic Devices for the Psychological Laboratory 145
BOUSFIELD, W. A., and BARRY, H. JR., The Visual Imagery of a Lightning Calculator 353
BOUSFIELD, W. A., VERVEER, E. M., and BARRY, H. JR., Change in Affectivity with Repetition 130
BURNS, M., and DALLENBACH, K. M., The Adaptation of Cutaneous Pain 111
CHANT, S. N. F., An Objective Experiment on Reasoning 282
CHAPMAN, D. W., Attensity, Clearness, and Attention 156
CHOU, S. K., What is the Curve of Forgetting? 348
COREY, S. M., The Dependence upon Chance Factors in Equating Groups 749
CROSLAND, H. R., and ANDERSON, I., A Method of Measuring the Effect of Primacy of Report in the Range of Attention Experiment 701
DALLENBACH, K. M., Tables for the Constant Methods 149
DALLENBACH, K. M., Ludwig Reinhold Geissler: 1879-1932 365
DALLENBACH, K. M., The Philadelphia Meeting of the Society of Experimental Psychologists 539
DALLENBACH, K. M., and BERGMAN, L. V., Portraits Useful to the Psychologist 165
DALLENBACH, K. M., and BURNS, M., The Adaptation of Cutaneous Pain 111
DALLENBACH, K. M., and EARHART, R. H., The Response of Warm Spots under Successive Stimulation 722
DALLENBACH, K. M., and THORNE, F. C., Spatial Perception in the Region of the Optic Disk 453
DAVIS, F. C., Aesthetic Proportion 298
DAVIS, H., and SAUL, L. J., Auditory Action Currents 358
DES BANCELS, J. L. The Curve of Forgetting 758
DESILVA, H. R., A Useful Periodical on Instruments 149
DIMMICK, F. L., A Simple Campimeter 148
DIMMICK, F. L., The Dependence of Auditory Experience upon Wave Amplitude 463
DIMMICK, F. L., and MCMICHAEL, G., The Psychophysical Determination of the Limits of Pure Gray 313
DRURY, M. B., Progressive Changes in Non-Foveal Perception of Line Patterns 628

DOLL, E. A., Psychological Significance of Cerebral Birth Lesions 444
EARHART, R. H., and DALLENBACH, K. M., The Response of Warm Spots under Successive Stimulation 722
FERREE, C. E., and RAND, G., A Device for Varying and Controlling the Entrance Pupil 329
FERREE, C. E., and RAND, G., The Glareless Lighting of a Dark Room 735
FERREE, C. E., RAND, G., and HARDY, C., An Important Factor in Space Perception in the Peripheral Field of Vision 228
FLEISCH, A., A New Method for Registering Time-Intervals 335
FORBES, T. W., The Hathaway Galvanic Apparatus as an Instrument of Research 502
FORBES, T. W., and LANDIS, C., A Critique of Crosland's "Measurement of Emotion" 522
FORBES, T. W., and MAYS, L. L., A Device for Automatically Varying a Series 343
FREEMAN, G. L., The Facilitative and Inhibitory Effects of Muscular Tension upon Performance 17
FREEMAN, G. L., A Simplified Heat Grill 344
FRY, G. A., Modulation of the Optic Nerve-Current as a Basis for Color-Vision 488
FRY, G. A., Color Phenomena from Adjacent Retinal Areas for Different Temporal Patterns of Intermittent White Light 714
GILLETTE, A., and WASHBURN, M. F., Motor Factors in Voluntary Control of Cube Perspective Fluctuations and Retinal Rivalry Fluctuations 315
GLANVILLE, A. D., The Psychological Significance of the Horopter 592
GOLDTHWAIT, C., Relation of Eye-Movements to Visual Imagery 106
GUILFORD, J. P., and SPENCE, W., The Affective Value of Combinations of Odors 495
GUILFORD, J. P., WALTON, W. E., and GUILFORD, R. B., Color Preferences of 1279 University Students 322
GUILFORD, R. B., WALTON, W. E., and GUILFORD, J. P., Color Preferences of 1279 University Students 322
GURNEE, H., Some Observations on After-Images of Bodily Movement 528
HALLETT, W. N., A Psychophysical Study of Visual Gestalten 691
HARDY, C., FERREE, C. E., and RAND, G., An Important Factor in Space Perception in the Peripheral Field of Vision 228
HARRIMAN, P. L., Intelligence and Handedness 526
HARTMAN, G. W., Insight *vs.* Trial-and-Error in the Solution of Problems ... 663
HARTMAN, M. L., and REYMERT, M. L., A Qualitative and Quantitative Analysis of a Mental Test 87
HELSON, H., Dr. Wilcox on "The Rôle of Form in Perception" 171
HELSON, H., A Child's Spontaneous Reports of Imagery 360
HOLLAND, B. F., Special Apparatus in the Laboratory of Educational Psychology 139
HULL, C. L., The Meaningfulness of 320 Selected Nonsense Syllables 730
HUNT, W. A., The Meaning of Pleasantness and Unpleasantness 345
ISRAEL, H. E., and SIIPOLA, E. M., Habit-Interference as Dependent upon Stage of Training 205
JENKINS, J. G., A Simple Tachistoscope of Many Uses 150
JENKINS, J. G., The Effects of Distraction 173
JENKINS, J. G., Instruction as a Factor in 'Incidental' Learning 471
JENKINS, J. G., and MOEDER, W. D., A Condenser-Charge Timer 742
JENKINS, J. G., and PAVLICEK, G., Paradoxical Warmth 350
JERSILD, A. T., The Constancy of Certain Behavior Patterns in Young Children 125
LANDIS, C., and FORBES, T. W., A Critique of Crosland's "Measurement of Emotion" 522
LAUER, A. R., Improvement and Standardization of the Pursuitmeter Technique 512
LUCKIESH, M., and MOSS, F. K., A Demonstrational Test of Vision 135
MARTIN, M. F., Responses of Animals to the Airplane 530
MAYS, L. L., and FORBES, T. W., A Device for Automatically Varying a Series 343
MCGEOCH, J. A., Curves of Memorization after Different Amounts of Practice 678

McMICHAEL, G., and DIMMICK, F. L., The Psychophysical Determination of the Limits of Pure Gray 313
MITCHELL, M. B., Errors in the Memorization of Numbers 1
MITCHELL, M. B., The Effect of Serial Position in the Continuous Memorization of Numbers 493
MOEDER, W. D., and JENKINS, J. G., A Condenser-Charge Timer 742
MOSS, F. K., and LUCKIESH, M., A Demonstrational Test of Vision 135
MUSGRAVE, H., and RUHL, R. G., A Demonstrational Apparatus for the Patellar Tendon Reflex 740
NAFE, J. P., Edgar James Swift: 1860-1932 364
NEWMAN, S. S., Further Experiments in Phonetic Symbolism 53
OGDEN, R. M., Gestalt Psychology and Behaviorism 151
PATERSON, D. G., The Fortieth Annual Meeting of the American Psychological Association 174
PATERSON, D. G., and RUNDQUIST, E. A., The Occupational Background of Feeble-Mindedness 118
PAVLICEK, G., and JENKINS, J. G., Paradoxical Warmth 350
PERL, R. E., The Influence of a Social Factor upon the Appreciation of Humor 308
PESSIN, J., The Comparative Effects of Social and Mechanical Stimulation on Memorizing 263
PETERSON, J., The Twenty-eighth Annual Meeting of the Southern Society for Philosophy and Psychology 530
PETERSON, J., A Correction 758
PETRAN, L. A., Pitch Discrimination near the Upper and Lower Thresholds of Audition 248
PILLSBURY, W. B., The Wundt Centenary 176
PRATT, C. C., The Time-Error in Psychophysical Judgments 292
RAND, G., and FERREE, C. E., A Device for Varying and Controlling the Entrance Pupil 329
RAND, G., and FERREE, C. E., The Glareless Lighting of a Dark Room 735
RAND, G., FERREE, C. E., and HARDY, C., An Important Factor in Space Perception in the Peripheral Field of Vision 228
REYMERT, M. L., and HARTMAN, M. L., A Qualitative and Quantitative Analysis of a Mental Test 87
RUCKMICK, C. A., The Eighth Annual Meeting of the Midwestern Psychological Association 752
RUHL, R. G., and MUSGRAVE, H., A Demonstrational Apparatus for the Patellar Tendon Reflex 740
RUNDQUIST, E. A., and PATERSON, D. G., The Occupational Background of Feeble-Mindedness 118
SAUL, L. J., and DAVIS, H., Auditory Action Currents 358
SCHEIDEMANN, N. V., A Novel Experiment with the Negative After-Image .. 361
SCHEIDEMANN, N. V., An Experiment in Reasoning 521
SIIPOLA, E. M., and ISRAEL, H. E., Habit-Interference as Dependent upon Stage of Training 205
SMITH, D. L., and WASHBURN, M. F., Stereoscopic Binocular Fusion in the Original Impression and in the Negative After-Image 320
SONTAG, L. W., and WALLACE, R. F., An Apparatus for Recording Fetal Movement 517
SPENCE, W., and GUILFORD, J. P., The Affective Value of Combinations of Odors 495
STAGNER, R., Improved Norms for Four Personality Tests 303
TAIT, E. F., A Reciprocal Reflex System in the Accommodation-Convergence Relationships 647
THORNE, F. C., A Thyratron-Tube Chronaximeter 515
THORNE, F. C., and DALLENBACH, K. M., Spatial Perception in the Region of the Optic Disk 453
VARON, E. J., and BENTLEY, M., An Accessory Study of "Phonetic Symbolism" 76

VERVEER, E. M., BARRY, H. JR., and BOUSFIELD, W. A., Change in Affectivity with Repetition 130
WALLACE, R. F., and SONTAG, L. W., An Apparatus for Recording Fetal Movement 517
WALTON, W. E., GUILFORD, R. B., and GUILFORD, J. P., Color Preferences of 1279 University Students 322
WARD, L. B., and BENNETT, G. K., A Model of the Synthesis of Conditioned Reflexes 339
WASHBURN, M. F., and GILLETTE, A., Motor Factors in Voluntary Control of Cube Perspective Fluctuations and Retinal Rivalry Fluctuations 315
WASHBURN, M. F., and SMITH, D. L., Stereoscopic Binocular Fusion in the Original Impression and in the Negative After-Image 320
WITTELS, F., Revision of a Biography 745
WOODROW, H., Individual Differences in the Reproduction of Temporal Intervals 271
WOODROW, H., Weight-Discrimination with a Varying Standard 391

BOOK REVIEWS

(The reviewer's name appears in parentheses after the title of the work.)

ADAMS, G., Psychology: Science or Superstition? (S. W. Fernberger) 190
ANDERSON, J. E., and others, Handbook of Child Psychology (C. M. Tryon) 368
ANON., I Lost My Memory: The Case as the Patient Saw It (M. F. Washburn) 575
ARMITAGE, D. M., A Challenge to Neurasthenia (K. Jensen) 567
AVELING, F., Personality and Will (C. N. Rexroad) 383
BAHLE, J., Zur Psychologie des musikalischen Gestaltens (C. A. Ruckmick) 555
BAUR, E., FISCHER, E., and LENZ, F., Human Heredity (F. O. Smith) 768
BENEDICT, A. E., Children at the Crossroads (J. Peterson) 201
BINGHAM, W. V. D., and MOORE, B. V., How to Interview (J. Peterson) .. 180
BLACKFAN, K. D. (Chairman), Growth and Development of the Child, Part IV. Appraisement of the Child: I. Mental Status. II. Physical Status (H. S. Conrad) 772
BLEULER, E., Naturgeschichte der Seele und Ihres Bewusstwerdens (M. F. Meyer) 564
BOAS, E. P., and GOLDSCHMIDT, E. F., The Heart Rate (J. Peterson) 563
BÖHME, J., Die Seele und das Ich im Homerischen Epos, mit einem Anhang Vergleich mit dem Glauben der Primitiven (M. J. Zigler) 193
BOLDYREFF, T. W., By Word of Mouth: Informal Essays on Language and Psychology from a Teacher's Point of View (J. Peterson) 575
BONAVENTURA, E., Psicologie dell'eta evolutiva (Infanzia e adolescenza) (J. Peterson) 557
BOOK, W. F., Economy and Technique of Learning (J. G. Jenkins) 547
BRITAN, H. H., The Affective Consciousness (R. T. Wiltbank) 549
BROTEMARKLE, R. A. (Ed.), Clinical Psychology. Studies in Honor of Lightner Witmer (S. J. Beck) 384
BRUCKNER, A., Das Problem der Schülerbeurteilung (M. L. Reymert) 553
BUCKE, R. M., Cosmic Consciousness: A Study in the Evolution of the Human Mind (M. B. Jensen) 202
BURNHAM, W. H., The Wholesome Personality (J. H. Edds) 574
BURROWS, C. N., Criminal Statistics in Iowa (K. B. Hait) 566
CAHEN, A., Statistical Analysis of American Divorce (S. N. F. Chant) 381
CARLSON, H. S., Information and Certainty in Political Opinions: A Study of University Students during a Campaign (C. Bird) 569
CHRISTIANSEN, B., Die Kunst (R. S. Hill) 196
CRAWSHAW, W. H., The Indispensable Soul (F. P. Boswell) 568
CREED, R. S., DENNY-BROWN, D., and others, Reflex Activity of the Spinal Cord (C. W. Brown) 780
CRONER, E., Die Psyche der weiblichen Jugend (C. M. Diserens) 551

CUVILLIER, A., Cours de philosophie. I. Manuel de philosophie à l'usage des classes de philosophie et de première supérieure (H. Sanborn) 203
DUNLAP, J. W., and KURTZ, A. K., A Handbook of Statistical Nomographs, Tables, and Formulas (M. B. Jensen) 193
DUNLAP, K., Habits, their Making and Unmaking (S. M. Corey) 761
FISHER, V. E., and HANNA, J. V., The Dissatisfied Worker (P. Blanchard) .. 571
FRYBERGER, H. E., The Abolition of Poverty (H. E. Cooper) 571
FRYER, D., The Measurement of Interest (H. E. Burtt) 559
GARNETT, A. C., The Mind in Action: A Study of Motives and Values (S. M. Newhall) 375
GARTH, T. R., Race Psychology (L. H. Lanier) 183
GATES, A. I., Interest and Ability in Reading (F. C. Davis) 567
GAULT, R. H., Criminology (H. L. Koch) 767
GERBERICH, J. R., A Personnel Study of 10,000 Iowa High School Seniors (J. C. Peterson) 371
GOODENOUGH, F. L., Anger in Young Children (S. M. Corey) 380
GROLLMAN, A., The Cardiac Output of Man in Health and Disease (C. W. Brown) 573
GRUDIN, L., Mr. Eliot Among the Nightingales (J. Peterson) 566
HAMILTON, J. A., Englisch-Deutsch für Psychologen (C. A. Ruckmick) 389
HANAWALT, E. M., Whole and Part Methods in Trial and Error Learning (E. M. Cook) 565
HARRIS, F. M., Essays on Marriage (R. A. Brotemarkle) 203
HASTINGS, M. L., Clasificación y estudio estadístico de 3,719 alumnos, de las escuelas secundarias de la cuidad de México por medio de exámenes mentales (T. R. Garth) 195
HENNING, H., Psychologie der Gegenwart (R. B. MacLeod) 569
HICKS, J. A., Acquisition of Motor Skill in Young Children (S. S. Renshaw) .. 778
HIRSCH, N. D. M., Genius and Creative Intelligence (M. B. Jensen) 370
HISSONG, C., The Activity Movement (H. B. English) 202
HÖFLER, A., Psychologie (C. W. Fox) 564
HOLT, E. B., Animal Drive and the Learning Process: An Essay toward Radical Empiricism (J. Peterson) 540
HONZIK, C. H., Delayed Reactions in Rats (J. Peterson) 387
HUDDLESON, J. H., Accidents, Neuroses, and Compensation (M. B. Jensen) .. 778
HUNTER, E. L., A Sociological Analysis of Certain Types of Patriotism (J. E. Winter) 573
HUTTON, I. E., The Sex Technique in Marriage (K. M. Dallenbach) 572
JASTROW, J., The House That Freud Built (M. Bentley) 759
JOHNSON, B. J., Child Psychology (F. L. Goodenough) 574
JONES, E., Nightmare, Witches, and Devils (N. B. Cuff) 383
KAFKA, V., Sexualpathologie, für Mediziner, Juristen, und Psychologen (L. S. Selling) 554
KARPF, M. J., The Scientific Basis of Social Work: A Study in Family Case Work (S. C. Kohs) 766
KATZ, D., Der Aufbau der Farbwelt (M. F. Martin) 552
KEELER, C. E., The Laboratory Mouse: Its Origin, Heredity, and Culture (J. Peterson) 777
KENNEDY-FRASER, D., Education of the Backward Child (R. M. Drake) 567
KOCH, H., Das Generationsproblem in der deutschen Dichtung der Gegenwart (B. Tapper) 204
KRANEFELDT, W. M., Secret Ways of the Mind (M. H. Elliott) 576
KROH, O., Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwicklung (F. L. Goodenough) 568
LANGE-EICHBAUM, W., The Problem of Genius (J. Peterson) 574
LASKER, B. (Ed.), Jewish Experiences in America: Suggestions for Study of Jewish Relations with Non-Jews (J. Peterson) 571
LICKLEY, J. D., The Nervous System: An Elementary Handbook of the Anatomy and Physiology of the Nervous System (W. N. Chappell) 572

LIGHTHOLL, W. D., The Person of Evolution; The Outer Consciousness; the Outer Knowledge; The Directive Power (J. Peterson) 390
LIPMANN, O., Leistungsfähigkeit und Selbstbeanspruchung: Eine psychologische Betrachtung (M. F. Hausmann) 552
LOCKHARDT, E. G., Attitudes of Children Toward Law (F. E. Lowell) 570
LUCAS, D. B., and BENSON, C. E., Psychology for Advertisers (F. C. Davis) .. 561
MACINTOSH, D. C. (Ed.), Religious Realism (F. P. Boswell) 376
MAHAN, J. A., Marie Louise: Napoleon's Nemesis (L. S. Selling) 204
MAJOR, R. H., Classic Descriptions of Disease (L. Carmichael) 576
MARBE, K., Psychologie der Werbung (M. F. Meyer) 568
MARBE, K., Der Strafprozess gegen Philipp Halsmann: Aktenmässige Darstellung und Kriminalpsychologische Würdigung (R. B. W. Hutt) 572
MCFARLANE, D. A., The Rôle of Kinesthesis in Maze Learning (J. U. Yarborough) 569
MCLAUGHLIN, M. A., The Genesis and Constancy of Ascendance and Submission as Personality Traits (G. W. Allport) 779
MEAD, M., Growing Up in New Guinea: A Comparative Study of Primitive Education (R. S. Hill) 372
MIBAI, S., An Experimental Study of Apparent Movement (M. J. Zigler) 378
MONEY-KYRLE, R. E., The Development of the Sexual Impulses (R. B. W. Hutt) 390
MOSS, F. A., and HUNT, T., Foundations of Abnormal Psychology (M. F. Meyer) 200
MURCHISON, C. (Ed.), The Psychological Register: Vol. III (K. M. Dallenbach) 178
NATHAN, M., L'esprit et ses maladies (E. S. Conklin) 198
NISSEN, H. W., A Field Study of the Chimpanzee: Observations of Chimpanzee Behavior and Environment in Western French Guinea (C. Bird) 781
OLDEKOP, E., Über das hierarchische Prinzip in der Natur und Seine Beziehungen zum Mechanismus-Vitalismus-Problem (J. Peterson) 555
OLIVER, J. R., Psychiatry and Mental Health (R. B. W. Hutt) 781
OSBURN, W. J., and ROHAN, B. J., Enriching the Curriculum for Gifted Children (P. Cattell) 200
PATERSON, D. G., Physique and Intellect (R. H. Seashore) 189
PAULHAN, F., The Laws of Feeling (C. R. Griffith) 373
PRATT, C. C., The Meaning of Music: A Study in Psychological Aesthetics (R. S. Hill) 194
PRESSEY, S. L., and PRESSEY, L. C., Introduction to the Use of Standard Tests (J. H. Edds) 572
RASMUSSEN, A. T., The Principal Nervous Pathways (L. H. Lanier) 202
REHBERGER, G. E., The Mind: A Key to the Interpretation of Psychical Phenomena (J. E. Earl) 570
RICHARDS, E. L., Behavior Aspects of Child Conduct (H. S. Conrad) 565
RIGNANO, E., The Nature of Life (R. B. MacLeod) 197
ROBERTSON, P. W., Life and Beauty: A Spiritual Autobiography (J. Peterson) 769
ROBINSON, E. S., Association Theory To-day: An Essay in Systematic Psychology (H. B. English) 560
ROCHELEAU, C., and MACK, R., Those in the Dark Silence: The Deaf-Blind in North America (S. P. Hayes) 382
ROSSMAN, J., The Psychology of the Inventor: A Study of the Patentee (J. Peterson) 558
RUSSELL, G. O., Speech and Voice (L. E. Travis) 386
SANDERS, J. E., Safety and Health in Organized Camps (L. Ackerson) 202
SCHIEFFELIN, B., and SCHWESINGER, G., Mental Tests and Heredity, including a Survey of Non-Verbal Tests (J. Peterson) 374
SCHNEIDER, E., Psychoanalyse und Pädagogik (F. Fearing) 377
SINCLAIR, R. D., A Comparative Study of Those Who Report the Experience of the Divine Presence and Those Who Do Not (M. F. Nichols) 562

SMITH, J. J., Social Psychology: The Psychology of Attraction and Repulsion (C. Hall) 202
SORANTIN, E., The Problem of Musical Expression (L. H. Lanier) 776
SPENCER, W. W., Our Knowledge of Other Minds (M. C. Barlow) 203
STEINBERG, W., Soziale Seelenhaltungen (M. L. Reymert) 573
STONE, C. P., DARROW, C. W., and others, Studies in the Dynamics of Behavior (C. W. Brown) 544
STOUT, G. F., Studies in Philosophy and Psychology (C. R. Griffith) 197
STRAUS, E., Geschehnis und Erlebnis (M. F. Meyer) 198
STROMSTEN, F. A., The Development of the Gonads in the Gold Fish, Carassius auratus (L.) (C. P. Stone) 572
STRONG, E. K., Change of Interest with Age (M. B. Jensen) 775
SWIFT, E. J., The Jungle of the Mind (E. Murray) 760
TAEUSCH, C. F., Policy and Ethics in Business (H. G. Bishop) 388
THOMPSON, D., A Mind that was Different (J. Peterson) 385
THURSTONE, L. L., and JENKINS, R. L., Order of Birth, Parent-Age and Intelligence (E. J. Asher) 387
TOLMAN, E. C., Purposive Behavior in Animals and Man (P. T. Young) 177
TRAVIS, L. E., Speech Pathology (L. S. Selling) 378
TROLAND, L. T., Psychophysiology: III. Cerebration and Action (M. J. Zigler) 771
TROW, W. C., Educational Psychology (F. S. Breed) 562
VALENTINE, W. L. (Ed.), Readings in Experimental Psychology (C. W. Brown) 367
VERLAINE, L., L'âme des bêtes: quelques pages d'histoire (C. Burri) 379
VERNON, M. D., The Experimental Study of Reading (M. A. Tinker) 389
VITELES, M. S., Industrial Psychology (H. E. Burtt) 764
VOLD, G. B., Prediction Methods and Parole (K. C. Garrison) 390
VON RINTELEN, F. J. (Ed.), Philosophia Perennis. Abhandlungen zur ihrer Vergangenheit und Gegenwart. Festgabe Joseph Geyser zum 60 Geburtstag. Vol. I. Abhandlungen über die Geschichte der Philosophie. Vol. II. Abhandlungen zur systematischen Philosophie (H. Sanborn) 199
WARREN, H. C., and CARMICHAEL, L., Elements of Human Psychology (J. Buel) 188
WESTERMARCK, E., Ethical Relativity (E. A. Kirkpatrick) 763
WOODWORTH, R. S., Contemporary Schools of Psychology (C. R. Griffith) ... 182
WOODWORTH, R. S., Psychology (K. M. Dallenbach) 186
YERKES, R. M., and YERKES, A. W., The Great Apes: A Study of Anthropoid Life (K. M. Dallenbach) 192
YOUNG, E. L., A Philosophy of Reality (C. R. Griffith) 204
ZEININGER, K., Magische Geisteshaltung im Kindesalter und ihre Bedeutung für die religiöse Entwicklung (P. F. Finner) 201
ZIEHEN, T., Die Grundlagen der Charakterologie (O. Klineberg) 556

THE AMERICAN JOURNAL OF PSYCHOLOGY

THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded in 1887 by G. STANLEY HALL

Vol. XLV JANUARY, 1933 No. 1

ERRORS IN THE MEMORIZATION OF NUMBERS

By MILDRED B. MITCHELL, George School, Pa.

The purpose of this experiment was to determine the various types of errors that occur in the memorization of lists of three-place numbers, and the frequency of their occurrence.[1]

Few studies of this type have been made. Hamilton's study is a notable exception.[2] He used the multiple choice method with both human and infra-human subjects, and found it possible to classify their errors into 5 types. While there was a great deal of overlapping and individual variation, the frequency of the different types of errors tended to vary with the animal's position in the phyletic scale. Experimenters working in memory have made various provisions in scoring for different types of errors. For example, Robinson,[3] using four-place numbers in a memory experiment with human subjects, took into consideration the source of the response when incorrect. If the four-place number given was the next number in the list, it counted as one error only, while if it was more than one place removed from its correct position it counted as four errors.

While conducting an experiment on retroactive inhibition under hypnosis, the present writer observed that frequently in giving a three-digit response, the subject would give one digit correctly, and two digits from the next number.[4] While these were not always in their correct position within the three-place numbers, they were observed to be so more frequently than it seemed possible to attribute to chance alone. Position and the next number were observed, therefore, to be important factors in the response. As a result of these observations, and the suggestion of

* Accepted for publication October 15, 1931.

[1] From the Psychological Laboratory of Yale University. This study was conducted under the direction of Professor Edward S. Robinson.

[2] G. V. Hamilton, A study of perseverance reactions in primates and rodents, *Behav. Monog.*, 3, 1916, (no. 13), 1-65; A study of trial and error reactions in mammals, *J. Animal Behav.*, 1, 1911, 33-66.

[3] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 6), 18.

[4] M. B. Mitchell, Retroactive inhibition and hypnosis, *J. Gen. Psychol.*, 7, 1932, 343-359.

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

types of errors in learning obtained from Hamilton, as well as the importance of position obtained from Robinson, a record of the responses was made during an experiment on retroactive inhibition using waking suggestion.[5]

In the previous experiments[6] merely plus or minus records of the errors were recorded, but in the present experiment, the entire response of the subject was taken if it was incorrect in any respect. The errors were then classified according to the number of digits given in the response and analyzed with respect to number of correct digits, position of these digits, and relation to the next number in the list.

Apparatus, Materials, and Subjects

Apparatus. The memory drum used in this experiment was designed by McGeogh.

Material. Lists of 10 three-place numbers were used as material in this experiment.

Subjects. The *S*s were all Yale students. Five were undergraduates and six were graduate students, all from departments other than psychology.

Method and Procedure

The anticipation method of learning was used. *S* was seated in a comfortable armchair in front of the apparatus. He was told that the experiment consisted of learning lists of 10 three-place numbers. He was then given a typewritten sheet with a verbal request to read the following instructions for learning the numbers.

During the *first* trial you will study each number as it appears. In the succeeding trials, try to give the number aloud before it appears in the slit. Give each digit separately; *i.e.* give 123 as one, two, three, not as one twenty-three. There will be no time between successive trials. The last number will become your cue for the first number of the list. Try to anticipate as many as you can each trial. You will be through when you have anticipated the entire list correctly. Do you understand? No questions will be answered once the experiment is started.

The instructions were then expanded verbally until *S* was sure he knew what was expected of him.

E sat at a table dorsal-laterally to *S*, out of his field of vision but in view of the exposed numbers.

Every *S* was given a practice period of from 8 to 11 days depending upon his speed of memorization. The usual procedure was to have *S* learn 2 lists each day with a short rest period between. All the experimenting was done between October 27, 1930 and December 17, 1930. Every *S* was given the series at the same time of day, five days a week.

The *S*s learned a list of 10 three-place numbers; after a 40-sec. interval they learned a second list of numbers, and then after another 40-sec. interval they relearned the first list. Preceding each list, *S* was given suggestions regarding the efficacy of retroactive inhibition. In Condition A, he was given the direct suggestion that he *would not* be able to remember the first list after learning the second list. In

[5] M. B. Mitchell, Retroactive inhibition and waking suggestion, *J. Abn. Psychol. & Soc. Psychol.*, 27, 1932, 336-341.

[6] Mitchell, *op. cit.*, *J. Gen. Psychol.*, 7, 1932, 343-359.

Condition B, he was given the direct suggestion that he *would* be able to remember the first list after learning the second list. A cycle was arbitrarily considered as 8 sessions with the 2 conditions given in the order ABBAABBA.

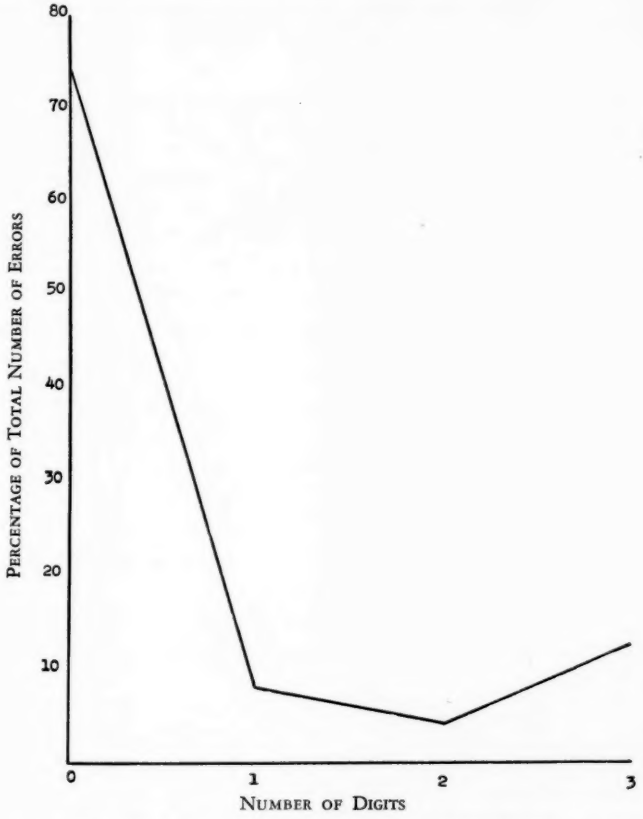


FIG. 1. GENERAL TYPE OF ERRORS FOR ALL *Ss*, ALL CYCLES, AND ALL LISTS (30,092 cases)

RESULTS

Classification of the errors. In anticipating a 3-place number *S* may make no verbal response, or may give one, two, or all the digits. Unless he gave all the digits in their correct order during the 2-sec. interval preceding their exposure, the trial was recorded as an error.

The percentage of each general type of error, classified according to the number of digits in the response, is shown in Fig. 1. These results are based on 30,092 errors. As Fig. 1 shows, there is a preponderance of 0-digit responses with the incorrect 3-digit responses ranking second in frequency. The 1-digit responses rank third and the 2-digit responses are least frequent of all.

Table I gives a summary of the two conditions for all *S*s in all lists, and all cycles. The gross number of errors for Condition B is 99.5% of those for Condition A, or 15,011 as compared with 15,081. The summary for each type of response is also practically identical as is shown most clearly by comparing the percentage of each type of error found under the two conditions. The greatest difference, 0.9% is found in the 1-digit responses. The percent of imperfect 3-digit responses is identical,

TABLE I

SHOWING FOR EACH CONDITION THE NUMBER AND PERCENTAGE OF ERRORS MADE BY ALL THE *S*s, IN ALL THE LISTS, DURING ALL THE CYCLES

Condition	Errors	No. Digits				Total no. errors
		0	1	2	3	
A	No.	11,262	1202	680	1931	15,081
	%	74.7	8.0	4.5	12.8	
B	No.	11,092	1341	652	1926	15,011
	%	73.9	8.9	4.3	12.8	

12.8%, for the two conditions. The reliability of the method is consequently assured—the statistical validity of the complete data is established by the equality of the two halves of the data. Henceforth, it will not be necessary to consider the two conditions separately.

Our next concern was with the errors made during the learning of the various lists, *i.e.* during the original learning of List 1, the learning of List 2, and the relearning of List 1. As Table II shows, the errors for the learning of List 2 and the relearning of List 1 are practically identical. The original learning of List 1 shows a few less 0-digit responses with a correspondingly greater number of imperfect 1-, 2-, and 3-digit responses. The total number of errors for List 2 is slightly greater than for the original learning of List 1, and the percentage of 0-digit responses is nearly 4% greater. On the whole, however, there is very little difference in either the number or type of errors found in the different lists,[7] so that the results from all lists may be combined in making comparisons between cycles and *S*s.

[7] A more detailed account is soon to appear in the *J. Exper. Psychol.* (Feb. 1933).

Our results thus far indicate that the relative percentages of the various types of errors is practically constant whether comparisons are made between conditions or lists. The next question was concerned with the effects of practice. This can most readily be examined by examining the results by cycles. A complete record was kept for the practice periods of

TABLE II

SHOWING FOR EVERY LIST THE NUMBER AND PERCENTAGE OF ERRORS MADE BY ALL THE *Ss* DURING ALL THE CYCLES

List	Errors	No. Digits				Total no. errors
		0	1	2	3	
1	No.	8,099	1069	561	1549	11,278
(original)	%	71.8	9.5	5.0	13.7	
2	No.	8.970	949	507	1438	11,864
	%	75.6	8.0	4.3	12.1	
1	No.	5,285	525	269	871	6,950
(relearning)	%	76.0	7.6	3.9	12.5	

5 of the *Ss*. Each of these *Ss* learned from 16 to 20 lists during the practice period, and 24 lists (including the relearning) during the first cycle. The top part of Table III is based on the 12,215 errors in the practice period and the 9,615 errors made in Cycle 1. With increase in practice there

TABLE III

SHOWING FOR EACH CYCLE THE NUMBER AND PERCENTAGE OF ERRORS MADE BY THE *Ss* IN ALL THE LISTS

Cycle	No. Ss	Errors	No. of Digits				Total no. errors
			0	1	2	3	
Practice	5	No.	9807	597	421	1390	12,215
		%	80.3	4.9	3.4	11.4	
1	5	No.	6692	1289	456	1178	9,615
		%	69.6	13.4	4.7	12.3	
1	6	No.	7103	280	299	1127	8,809
		%	80.6	3.2	3.4	12.8	
2	6	No.	4566	492	306	872	6,236
		%	73.2	7.9	4.9	14.0	
3	6	No.	3993	482	276	681	5,432
		%	73.5	8.9	5.1	12.5	

is a relative increase in the number of 1- and 2-digit responses, especially 1-digit responses, with a corresponding decrease in 0-digit responses. The same thing holds true for the three cycles run by the other 6 *Ss* (lower part Table III). Cycle 1 for the 5 *Ss* shows a higher percentage of 1-digit than 3-digit responses. For none of the three cycles is that true in the case

of the other 6 *Ss*. On the whole, these 5 *Ss* were slower in learning, *i.e.* made more errors, than the other 6. The further significance of this fact will be seen when the *Ss* are grouped according to speed of learning or number of errors.

The increase in the number of 1- and 2-digit responses with practice for the group of 6 *Ss* is more apparent than real. It is only the slower *Ss* who are making these changes while the faster *Ss* are keeping their relative frequencies practically constant, as may be seen in Table IV. With practice the slow *Ss*, instead of becoming more like the faster, tend to learn still more by *parts*, thus accentuating the differences between the two groups, and causing an apparent increase with practice in the total number of 1- and 2-digit responses for all *Ss*.

TABLE IV

SHOWING FOR EACH CYCLE THE NUMBER AND PERCENTAGE OF ERRORS MADE IN ALL LISTS BY *Bri*, THE FASTEST S, AND BY *Cob*, THE SLOWEST S IN THE GROUP OF 6 *Ss*

S	Cycle	Errors	No. of Digits				Total no. errors
			0	1	2	3	
Bri	1	No.	826	17	15	186	1044
		%	79.1	1.6	1.4	17.8	
	2	No.	673	14	7	150	844
		%	79.7	1.7	.8	17.8	
	3	No.	663	12	9	103	787
		%	83.6	1.6	1.2	13.6	
Cob	1	No.	1440	104	84	171	1799
		%	80.0	5.8	4.7	9.5	
	2	No.	802	187	98	145	1232
		%	65.1	15.2	8.0	11.8	
	3	No.	642	219	97	114	1072
		%	59.9	20.4	9.0	10.6	

Bri held first place for speed of learning and lowest number of errors in all three cycles. For the 6 *Ss* run for three cycles, *Cob* was just as consistently the slowest, and made the most errors. The relative number of 1- and 2-digit responses does not change with practice for *Bri* but stays practically identical throughout (Table IV). The relative number of 1- and 2-digit responses steadily increases with practice for *Cob* while the 0-digit responses steadily decrease (Table IV). In the second and third cycles the number of 1-digit responses exceeds the number of 3-digit responses. This is the same phenomenon already found with practice in the relatively slow group of 5 *Ss* (Table III).

There are large individual differences in the total number of errors made and in the relative frequency of each type of error. For instance, the three

slowest *Ss* each have more than twice as many errors as the fastest *S. Bri* has very few 1- and 2-digit responses (Table V) while the slowest *S, Hic,* has almost as many 1-digit as 3-digit responses.[8] When the 5 *Ss* below the median speed are compared with the 5 above, the same holds true (Table V). There are relatively more 1- and 2-digit and less 0- and 3-digit responses for the slow group than for the fast. Apparently the slower *Ss* are learning the three-place numbers by *parts* so that they give more 1- and 2-digit responses than the more successful memorizers; the latter tend to give 3-digit or 0-digit responses almost exclusively. They seem to be learning the numbers as *wholes.* With the fastest *S, Bri,* this remained true even after much practice. A slower *S, Cob,* on the contrary, with practice tended to give relatively fewer 0-digit responses, to speak forth

TABLE V

SHOWING THE DIFFERENCES BETWEEN THE SLOW AND FAST *Ss* IN THE NUMBER AND PERCENTAGE OF ERRORS MADE IN ALL LISTS DURING CYCLE 1

Ss	Errors	No. of Digits				Total no. errors
		0	1	2	3	
Bri (fastest)	No.	826	17	15	186	1,044
	%	79.1	1.6	1.4	17.8	
Hic (slowest)	No.	1704	252	110	298	2,364
	%	72.1	10.7	4.7	12.6	
Fast Group	No.	5277	160	201	1052	6,690
	%	78.9	2.4	3.0	15.7	
Slow Group	No.	7498	1051	480	1153	10,182
	%	73.6	10.3	4.7	11.3	

the one and two digits he had learned, (Table IV). Hence practice tends to accentuate the earlier individual differences in method of learning numbers.

In general, the most frequent response by far of all the imperfect responses is the 0-digit response with the 3-digit response holding a distant second place. On the whole, the 2-digit response is least frequent of all. In *Ss* who learn very rapidly and apparently by the whole method, the 1- and 2-digit responses are, however, almost the same in frequency. On the contrary, with *Ss* who learn more slowly, the 1-digit responses ap-

[8] The *Ss* with the fewest errors learned the most rapidly, and vice versa. There was a correlation of 0.92 between the total number of trials to learn the lists and the total number of errors in the first cycle for the 10 *Ss* used in comparing slow with fast *Ss*. There was practically no difference in overlearning scores; the slow group had a mean score of 79.1 while the fast group had a mean score of 79.3. There was only the slight positive correlation of 0.28 between the overlearning scores and number of errors.

proach the 3-digit responses in frequency and this difference is accentuated with practice. Of course these differences are for small highly selected groups and might not hold for the population in general.

Analysis of 1-digit responses. In giving a 1-digit response there are always 10 possibilities. By pure chance or guess work any one of the 10 digits may be given. Since there are three digits—all different—in each number, the probability of getting one of these, that is, of making a correct 1-digit response is 3 out of 10 or 30%. If for convenience of illustration the excluded numbers 456 and 789 be considered the correct and next number respectively, then the probability of an *S* giving 4, 5, or 6 is 30%. The chance that the digit given is not only correct, but also in the correct position, that is, the first digit in the number (as 4 in the example), is one out of 10 or 10%. In other words, the probability that the correct 1-digit response will be in the wrong position (*i.e.* will be 5 or 6) is 2 out of 3 or 66.7%. There were a total of 2543 1-digit responses made by the 11 *S*s during the experiment. Out of these 1939 or 76.2% were correct, while as stated above, only 30% would be correct by chance alone. Furthermore, only 258 or 10.1% of these were in a wrong position while 66.7% might have been had chance alone been operating. This is 56.6% less than chance would give. Thus in about half of the cases, *S* actually knew the correct response is so far as he went and did not get it correct by chance alone. He had learned a *part* of the correct response.

Of the incorrect responses, the probability of being one of the three digits in the next number (as 7, 8, or 9) is again 3 in 10 or 30%. Out of the 604 incorrect responses, 420 or 69.5% were from the next number (Table VI). This is more than twice the probability from pure chance. Instead of 66.7% of these being in the wrong position only 25.2% were other than the first digit in the next number (as 7 in the example given above).

In all such analyses there is some factor other than chance operating that gives responses identical with those in the next number. Of course, the three correct digits are ruled out, for if they are given, the response would not be among those considered here. A further complicating factor is that the preceding number is exposed at the time the response is given. Its digits might tend to act as a positive suggestion to give one or more of them, or on the other hand, they might act as a negative suggestion to avoid giving them. The determination of these factors is a problem in itself that cannot be considered in detail here, but might be determined

from the data of this experiment.[9] Apparently, these factors are more or less balancing each other or at least are not all important factors for the numbers are not only frequently identical with those in the next number, but are also in the correct position within the next number.

A comparison of the 5 *S*s who learned most slowly shows about 10% more correct responses for the slow group than for the fast and about 6% less position errors. The influence of the next number was the same in both cases, about 69%. These same tendencies are seen in an exaggerated form when the fastest and slowest *S*s (for all the three cycles) are compared. Here the slowest has about 20% more correct responses and 10% less position errors. In addition, the source of the incorrect responses in the next number is found in 23% more cases in the slowest *S* than in the

TABLE VI

AN ANALYSIS OF 1-DIGIT RESPONSES FOR ALL *S*s IN ALL LISTS DURING ALL CYCLES (2543 Cases)

	Correct	W.P.*	Incor.	Next no.	W.P.*
No.	1939	258	604	420	106
%	76.2	10.1	23.8	16.5	4.2
% W.P.*		13.3			25.2
% n†				69.5	

* Wrong position, means that the digit is correct but in the wrong position within the number and when heading a column indicates the number of position errors among the responses in the preceding column.

† The % of the digits given incorrectly that are found in the next number.

fastest. Evidently, the slower *S*s actually are learning the numbers a digit at a time and so are more successful in their 1-digit responses. The faster *S*s may either be guessing or starting to give a longer response and stopping as they realize they have made an error.

It may be concluded that there is some evidence for digits being learned separately especially by the slower subjects since 1-digit responses are more frequently correct than chance would lead one to expect. Their position is learned at the same time because the number of position errors is much less than chance alone would allow. Furthermore, the next number is an important source from which incorrect 1-digit responses are obtained.

Analysis of 2-digit responses. In making a 2-digit response there are three gross possibilities regarding its degree of correctness: both digits may be correct, one digit may be correct, or both may be incorrect. Any 2-digit

[9] An actual analysis of all the responses with respect to the preceding number (the one exposed) showed approximately a chance appearance of these digits; hence they were not avoided.

response containing 4, 5, or 6 as one of its digits would be of the type of 1-digit correct, while all others would be of the 0-digit correct type. Taking all *Ss* for the entire experiment, 45% of the 2-digit responses were correct so far as digits alone were concerned. (See Table VII). Thus if only two digits were given, the chances were about even that at least one of these digits would be incorrect. As a matter of fact, of all the 2-digit responses one of the digits was correct nearly as frequently as two of them, while only a relatively small percentage had both digits incorrect.

When the 5 slow *Ss* are compared with the 5 fast *Ss* for the first cycle, the rank order for the number of correct digits is the same as for all *Ss* taken together in Cycle 1. The faster *Ss* give slightly fewer 2-digit correct and

TABLE VII

AN ANALYSIS OF 2-DIGIT RESPONSES FOR ALL *Ss* IN ALL LISTS DURING ALL CYCLES (1337 Cases)

	2 cor.	W.P.	1 cor.	W.P.	1 in next no.	W.P.	2 incor.	2 in next no.	W.P.	1 in next no.	W.P.
No.	601	249	568	119	298	152	168	125	20	31	12
%	45		42.5				12.6				
% W.P.*		41.4		21		51			16		38.7
% n †					52.5			74.4		18.5	

* Wrong position and when heading a column indicates the number of position errors among the responses in the preceding column.

† Responses from the next number.

more 1-digit correct responses.[10] This is in accordance with the hypothesis set forth by the writer in the previous sections; namely, that the slower *Ss* are learning the numbers by parts and hence have more of their partial responses correct.

The permutations of ten digits taken two at a time is 90; therefore, there are 90 possible 2-digit responses.[11] The permutations of three digits taken two at a time is 6; therefore, there are 6 correct 2-digit responses possible as illustrated above. In other words, the probability of getting a 2-digit response correct by mere chance is 6 in 90 or 6.7%. Actually, however, out of the 1337 2-digit responses (Table VII) 601 or 45.0% were correct. Of the 90 permutations only one can be correct with respect to position as well as digits. Hence of the 6 correct responses only 1 in 6 (45 in the example where the correct number is 456) would have the correct position by chance; in other words, 83.3% could be expected to

[10] Here as throughout this paper comparisons are in terms of percentages not in actual numbers.

[11] Not counting responses in which one digit is repeated. These practically never occurred probably partially due to the fact that the numbers to be learned never contained a repetition of digits.

have a wrong position. Only 41.4% or less than one-half that number were in the wrong position.

The case of one digit being correct here is the same as when it is the only digit given. Instead of obtaining 30.0% correct, there were 42.5%; hence, there was slightly more than a chance success. In order to avoid a position error the first digit would have to be given as the first one of the pair, or the second one as the second digit of the pair. For instance, in the example 456, 42 would have the first digit correct and in the correct position, while 15 would have the second digit correct and in the correct position. Instead of 66.7% position errors only 21.0% were obtained.

If chance should be credited with giving only 6.7% 2-digits correct responses and 30.0% 1-digit correct responses, it would leave 63.3% with 0-digits correct. In contrast with this, only 12.6% fall within this classification.

With 1-digit correct, the probability is 30.0% of the other digit being one of the three digits from the next number (7, 8, or 9 in the example), but 52.5% were from the next number. 51.% of these were in the wrong position out of a probable 66.7%. Here again the differences are slightly greater than chance.

The largest variations from chance come when two of the incorrect digits are from the next number in the list. The probability is only 6.7%, but 74.4% of these incorrect responses are both correct for the next number. A large variation is also found with respect to the position of the digits. Thus while the probability of getting them in the wrong position is 83.3%, only 16.0% were incorrectly placed.

The 2-next-number responses were so far from chance that the expected 30% probability of getting 1-digit from the next number was made impossible, but 18.5% of the cases did appear with one digit from the next number with 38.7% instead of 66.7% of these digits in an incorrect position.

In general, when 2-digit responses are given by the *S* at least half of them contain an error. Most of these contain only one incorrect digit half of which come from the next number. Of the 2-digit incorrect (0-digit correct) responses about three-fourths have both digits from the next number and most of them are in the correct position.

Analysis of 3-digit responses. In a 3-digit response, there are 4 possibilities with respect to the number of digits correct. All of the digits may be incorrect, one, two or three of the digits may be correct. The 3-digits correct are composed of those responses with the digits in the wrong posi-

tion and those responses given correctly but too slowly. The relative frequency of these types of errors for all *S*s, all lists, all cycles, may be seen in Table VIII. Nearly half, 44.5%, of all the 3-digit responses (not in-

TABLE VIII

AN ANALYSIS OF 3-DIGIT RESPONSES FOR ALL *S*s IN ALL LISTS DURING ALL CYCLES (3858 Cases)

	Too slow	W.P. in list	n. no. in list	Digits W.P.	o cor.	1 next no.	W.P.	2 next no.	W.P.	3 next no.
No.	615	800	477	440	504	27	16	52	25	411
%	15.9	20.7	12.4	11.4	13.1	.7	.4	1.3	.6	10.7
% W.P.				41.7			59.3		48.1	
% n			62.4			5.4		10.3		81.5

	W.P.	1 cor.	W.P.	1 next no.	W.P.	2 next no.	W.P.	2 cor.	W.P.	1 next no.	W.P.
No.	16	581	194	211	123	189	65	1718	481	825	425
%	.4	15.1	5.0	5.5	3.2	4.9	1.7	44.5	12.5	21.4	11.0
% W.P.	3.9		33.4		58.3		34.4		28.0		51.5
% n				36.3		32.5				48.0	

cluding the perfect responses) contain two correct digits; 3-digits correct ranks second with a frequency of about half as many. The 0- and 1-digit correct responses are both relatively few in number and about equal with a slight difference in favor of the 1-digit correct responses.

Just as formerly we have found significant differences when we have compared the slower with the faster *S*s, so here there is also a difference. While the 0- and 1-digit correct responses remain relatively low for both groups, they are lower for the slow group. There is a decided increase for 2-digits correct for both groups, but it also is lower for the slower group. The most marked difference comes in the 3-digit correct responses. For the fast group, there is a huge drop, down to below the number of 1-digit correct responses. In contrast to this, the slow group have even more 3-digit correct responses than two or any other. The ranking of the various types is almost completely disrupted, although the 0- and 1-digits correct remain comparatively low and the 2-digits high in both cases. These same differences are merely exaggerated when the slowest and fastest subjects are contrasted.

In the majority of cases when 0-digits are correct, the number is merely displaced within the list (W.P. in list, Table VIII). There were 800 such misplacements.[12] Since there were nine numbers each of these might have been, the probability by mere chance of their being the next number in

[12] These are further analyzed under the title of "Anticipatory place-skipping tendencies in the memorization of numbers" which is soon to appear in this JOURNAL.

the list was only 11.1%, but 62.4% were found to belong there. This same influence will be seen again when the total number of 3-digit responses with no correct digits is analyzed.

If no digits are correct, one, two, or three of them may come from the next number. The mere mathematical probability of obtaining digits from the next number would lead one to expect more with one from the next number than two, and more two than three since the probabilities are 30%, 9% and 0.1% respectively. However, the reverse is found to be true with only 5.4% of these responses with one, 10.3% with two, and 81.5% with three digits from the next number. The position errors were also reversed from the mathematical probability; they decrease rather than increase with the number of digits involved. For one digit in the next number 59.3% were in the wrong position which is a little less than 66.7%, the chance allotment. For two digits in the next number there were only 48.1% instead of 83.3% in the wrong position. For three digits in the next number only 3.9% out of a probable 99.2% were in the wrong position.

As stated in the introduction, while doing some experimenting on hypnosis using retroactive inhibition with lists of numbers, the writer noticed the *Ss* giving one digit correct with two digits from the next number, or two digits correct with one digit from the next number. The digits were not always in their correct position, but were observed to be so more frequently than could be attributed to chance. The next few paragraphs give a summary of the quantitative findings which confirm these observations.

In the cases in which one digit out of the three given was correct, there were only half as many position errors as could be expected, 33.4% out of a probable 66.7%. Chance could be held responsible for the 36.3% of single digits found in the next number. The actual number of 3-digit responses with two digits from the next number was nearly the same, 32.5%, but in this case the probable percentage was only 9; therefore, something more than chance is needed to explain the finding. Similarly, it takes more than chance to explain that only 34.4% of these digits were in the wrong position since chance would allow 83.3%. On the other hand, when only one digit came from the next number, it was mere chance if it were in the right position. There was, therefore, a tendency to obtain responses of one digit correct with two digits from the next number.

In the cases where two digits were correct, only 28.0% out of a probable 83.3% were in the wrong position. Practically half of the time, (48.0%) the remaining digit was from the next number although only

30% could be attributed to chance. Half of the time (51.5% wrong position) it was also in the correct position although chance would only account for it one-third of the time. There was, therefore, a tendency to obtain responses of two digits correct, with respect to position as well as digits, with one digit in the correct position from the next number.

With a retroactive inhibition set-up such as that used in this experiment, it would seem reasonable to expect some of the responses in the learning of List 2 and the relearning of List 1 to be numbers from the list just learned. In learning List 2, 7.6% of the incorrect 3-digit responses were numbers from List 1. In the relearning of List 1, 13.5% of the incorrect 3-digit responses were numbers from List 2. The difficulty in evaluating the influence of a previously learned list is the fact that with only 10 digits there is so much repetition within two lists that no adequate account can be taken of any responses not given completely. For instance, any two digits may have occurred several times within the same number in either list; it would, therefore, be impossible to say that the response was due to the number in the previous list.

It is of some interest in view of the similarity theory of retroactive inhibition as expounded by Robinson[13] and Skaggs[14] to note the number of digits correct in the responses taken over bodily, as it were, from the preceding list. Of the 227 responses of this kind, 119 or 52.4% of them contained two correct digits (Table IX) while only 8 or 3.5% had no

TABLE IX

SHOWING THE NUMBER AND PERCENTAGE OF ERRORS FOR EACH NUMBER OF DIGITS CORRECT IN ALL THE 3-DIGIT RESPONSES AND THOSE FROM THE PRECEDING LIST DURING ALL CYCLES FOR LIST 2 AND RELEARNING OF LIST 1

3-digit responses	Errors	No. Digits Correct				Total no. errors
		0	1	2	3	
From Preceding List	No.	8	72	119	28	227
	%	3.5	31.7	52.4	12.3	
Total	No.	305	376	1021	607	2309
	%	13.2	16.3	44.2	26.3	

correct digits. This is in decided contrast with the 13.2% of 0-digits correct found among all the 3-digit responses for these two lists. There was almost twice as large a percentage of 1-digit responses correct for preceding-list errors as for all such responses for these lists. The apparent

[13] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.* 28, 1920, (no. 128), 1-57; The similarity factor in retroaction, this JOURNAL, 39, 1927, 297-312.

[14] E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, (no. 161), 1-60.

differences in the 3-digits correct is due to the fact that the "too slow" responses are included in the 607 3-digit correct responses. When these are excluded the percentage is reduced to 10. There was no possibility of a large frequency of the 3-digits correct from the preceding list because it involved the relatively rare phenomenon of having numbers in two succeeding lists containing the same three digits. From these tabulations, it can be stated that when a response identical with a number in the preceding list is given, that number is almost always partially identical with the correct number and in half of the cases it has two out of three digits identical.

Summary and Conclusions

The results as indicated in the preceding sections may be summarized as follows.

(1) The most frequent type of error in learning lists of 10 three-place numbers is to give no verbal response, that is, a 0-digit response. The 3-digit response holds a distant second place with the 1-digit response ranking third and the 2-digit response least frequent of all. This is true even after having just learned another list and for the relearning of a list. Practice tends to increase the relative frequency of 1-digit responses and correspondingly decrease the number of 0-digit responses. For slow *Ss* this tendency is so marked that the 1-digit responses sometimes become more frequent than the 3-digit responses, but this is not true for the fast *Ss*.

(2) The numbers are sometimes learned by digits especially by the slower *Ss* since more correct 1-digit responses are given and given in the correct position than can be attributed to chance.

(3) Approximately half of the 2-digit responses contain one incorrect digit. This incorrect digit comes from the next number about half of the time. The relatively few cases with both digits incorrect usually have the next number as their source.

(4) Considering all the *Ss*, 3-digit responses have 2 of the digits correct more frequently than 0, 1, or 3. When the fast *Ss* are considered separately this is true in an exaggerated form but when the slow *Ss* are considered alone, the 3-digits correct are the most frequent. There is a tendency for the 1-digit correct responses to be completed with two digits from the next number, and for 2-digit correct responses to be completed with one digit from the next number. A number from a list just previously learned is practically never given unless it is partially identical with the correct number, and most frequently has two digits in common.

The fact that the *S*s who learned most rapidly tended to give zero or three digits rather than one or two digits suggests the possibility of improving speed of memorization in the slower *S*s by giving explicit directions about learning the number as a whole rather than learning and reciting it by parts. This might be determined experimentally by varying the instructions for memorization.

All *S*s tend to give the digits in their proper sequence even when they are not correct but belong in the next number. The tremendous influence of the next number found throughout all the verbal responses may be described as a form of retroactive inhibition within the list. The *S*'s chances of remembering a particular three-place number exposed to him for two seconds at a time seems to be lowered when he is shown a similar three-place number immediately following the exposure. It may also be described as a forward association within the list or a place-skipping association.

THE FACILITATIVE AND INHIBITORY EFFECTS OF MUSCULAR TENSION UPON PERFORMANCE

By G. L. FREEMAN, Northwestern University

To critical students, it is now quite evident that the relationship between mental activity and muscular tension is far from simple. One of the most perplexing problems of contemporary research concerns the universal validity of muscular reënforcements. Data obtained under widely different conditions indicate that increments in tension may (1) increase efficiency of performance, (2) have no effect on it, or (3) be actually inhibitory. Such apparently contradictory results point to the necessity of further work, both with more refined techniques and with more rigid experimental controls.

Our general problem has thus been formulated as follows: When may muscular tension facilitate performance and when is it likely to be inhibitory? This paper describes several new approaches to the question and attempts to clarify the interpretation of data obtained from other sources. Since the terms facilitation and inhibition have both a general and a technical meaning, it is advisable to mention at the outset which concept is to be used. According to the general view all factors, environmental or organic, which influence the efficiency of performance favorably or unfavorably are termed facilitators and inhibitors respectively. While this non-technical view is implied in the treatment of our problem, its relation to physiological concepts will be attempted in a final section.

The effect of muscular tension upon mental processes has been approached experimentally from two directions. The first, which has occupied the majority of researchers, is to vary some phase of behavior and observe its influence upon the tonus accompaniments of the work. The second attack is to alter the tension of the subject experimentally and observe its effect upon performance.

The validity of investigations of the first type depend primarily upon the adequacy of the technique used to record the changes in tonus. These techniques I

* Accepted for publication October 10, 1931. From the Psychological Laboratory of the Institute of Human Relations, Yale University. This research was made possible by a grant from the National Research Council. The writer acknowledges with pleasure the assistance and encouragement received from Professor Raymond Dodge.

have critically examined in a previous article.[1] In general, it may be said that the mechanical systems seem the more open to exploitation than the electrical systems at the present time; and that in such systems (mechanical) the problem of measuring tonus changes resolves itself into one of optimal magnification with a minimum of distortion. Since most existent work is open to technical criticism, it is all the more significant to find a certain general agreement. Even with such a crude indicator as the amplitude of knee-jerk, Lombard,[2] Tuttle,[3] and many others[4] have reported an increase in tension during mental activity. But whether such tension is only an incidental consequence of cerebration or an actual reënforcing agent is not apparent from such data as these. In order to answer this delicate question, more careful experimental controls were devised. Golla was among the first to utilize more refined means of recording and to compare extent of tonus increment with work output.[5] In his second article (Golla and Antonovitch)[6] he noted that the maximal tension usually preceded the actual mental work. Although the work was said to become progressively more difficult, the period of heightened tension was not prolonged. On the basis of such results, Golla argued that the tonus accompaniments of mental effort are merely "atavistic phenomena representing the preparation of the organism for continued bodily activity, the necessity for which no longer exists." Opposed to this interpretation are several extensive researches, utilizing the same mode of attack. Bills and Brown reported that tension in the arm muscles was greatest and work-output highest, when the subject was set to do a limited amount of work.[7] Freeman showed that where quadriceps tension increased under distraction, work output tended to be kept to normal.[8] The work of Freeman and Lindley indicated that tonus increases as the subject becomes relatively fatigued and that the greater the tonus increments during protracted work, the less the average decrement in work output.[9] Bills found that the tension of arm and jaw muscles increased when the subject was running the more difficult parts of the finger maze.[10] Duffy, however, reports that children with highest tension scores usually give the poorest performance.[11]

---

[1] G. L. Freeman, The measurement of tonus by deformation of the tendon, this JOURNAL, 42, 1930, 581-592.

[2] W. P. Lombard, The variations of the normal knee jerk and their relation to the activity of the central nervous system, *ibid.*, 1, 1887, 2-71.

[3] W. W. Tuttle, The distribution of tone in skeletal muscle, *J. Exper. Psychol.*, 8, 1925, 319-322.

[4] A fairly complete list is given by A. G. Bills, The influence of muscular tension on the efficiency of mental work, this JOURNAL, 38, 1927, 227-251.

[5] F. L. Golla, The objective study of neuroses, *Lancet*, 201, 1921, 118.

[6] F. L. Golla and S. Antonovitch, The relation of muscular tension and the patellar reflex to mental work, *J. Ment. Sci.*, 75, 1929, 234-241.

[7] A. G. Bills and C. Brown, The quantitative set, *J. Exper. Psychol.*, 12, 1929, 301-324.

[8] Freeman, Changes in tonus during completed and interrupted mental work, *J. Gen. Psychol.*, 4, 1930, 309-334.

[9] G. L. Freeman and S. B. Lindley, Two neuro-muscular indices of mental fatigue, *J. Exper. Psychol.*, 14, 1931, 567-605.

[10] A. G. Bills, Tension in learning and association, *Proc. 9th Int. Cong. Psychol.*, 1929, 75-76.

[11] E. Duffy, Tensions and emotional factors in reactions, *Genet. Psychol. Monog.*, 7, 1930, 1.

It is difficult to compare any of the above work. Golla's reports of his experiments are very sketchy and show little familiarity with accepted concepts of psychological control. He does not state how long his subject worked, what the instructions were, and whether or not a period of pre-work rest was given. With regard to his recording technique, he admits that "the delicacy of the method also showed slight postural adjustment;" but it is not clear what means, if any, were used to check or cancel the effects of these accidental movements on the records. While the work of Freeman and Lindley utilized an even more delicate system of recording tension, some check was obtained upon the effect of mechanical leg-displacements on the records. In common with other researches, however, very little was known of the absolute level of muscular tension from which increment occurred. Such an important matter was apparently not considered in the work of Bills, who used pressure downward on the pencil point as an indicator of the tension in the arm muscles, or of Duffy, who used pressure on a rubber ball held in the hand.

It is evident that aside from the question of the relative merit of recording techniques, none of these experiments provides crucial data upon the question of tonic facilitation. One difficulty is that there is no norm with which to compare the alleged facilitation.

The second type of attack owes its present popularity to the ambiguity of results obtained by the first. Here the general plan is to set up a state of heightened muscular tension experimentally and compare the efficiency of mental work during this period with a control series of observations. Various means of inducing tension have been used, including formal instructions to be relaxed or tense, and situational instructions, such as the gripping of dynamometers. The general outcome of this type of research has been in favor of muscular facilitation. Mental processes may be "relaxed away." Under muscular relaxation the knee-jerk is abolished (Jacobson).[12] Relaxation is accompanied by a reduction in the extent and speed of reaction to induction shock, as well as a raising of the sensory threshold (Miller).[13] Conversely, the rate of learning various tasks is facilitated by experimentally induced tension (Bills).[14]

The logic of this type of experimentation is based on the assumption that the *S* is more relaxed or tense in the test than in the control series of observations. But this is not a matter which can be settled arbitrarily. Some objective indicator of tension (such as is used in the first type of attack) is desirable as a check upon the actual neuro-muscular state of the organism in test and control series. Such a check has rarely been employed in the past.

There are many apparent difficulties which hinder the study of tonic muscular facilitation and inhibition at the higher levels of neural integration. In the first place we cannot deal with the neuro-muscular processes in a manner analogous to our treatment of external physical stimuli. If

[12] E. Jacobson, *Progressive Relaxation,* 1929, 112, 164.
[13] M. Miller, Changes in the response to electric shock produced by varying muscular conditions, *J. Exper. Psychol.,* 9, 1926, 26-45.
[14] Bills, *op. cit.*

we wish to ascertain the facilitating effect of sound upon reaction to light, we define accurately the amount of the facilitating stimulus as well as its time of presentation. This is difficult when the facilitating stimulus is to be the tonic contraction of muscle. True, we may have the *S* grip a dynamometer with a certain force at a certain time and observe the effect which this has upon something else he is doing. But if gripping the dynamometer becomes the *S*'s major problem, to the relative neglect of the task first assigned, an inhibition may be worked by a shift in the behavioral pattern to a more dominate competing tendency. In the second place, precise time relations are difficult to study except on the reflex level. Here we might hope to accurately record the intervals of optimal facilitation and inhibition. The patterns of cortically dominated performance cannot be isolated in such a manner. They involve many levels of neural action. So if evidence was obtained of direct facilitation or inhibition of reflex response, it would be difficult to carry the analogy to the more complicated pattern, where the facilitative effects might be indirect. Clearly, we cannot set up a simple situation such as is possible in most work on facilitation and inhibition. Our approach must be more indirect. Only by correlating variations in behavior and tension under a wide variety of experimental conditions can we hope for adequate data. Even then our interpretation will be largely a matter of inference, the validity of which depends in large measure upon the experimental precautions taken in obtaining the results. These precautions may be summarized under the following headings:

(*1*) *Measurement of tension.* A sensitive mechanical system is desirable in connection with all experiments. Whenever used it should be carefully checked to determine the effects of any artifacts (such as movement) in the records.

(*2*) *Pre-work baseline.* Since all records of tension are relative, care should be taken to establish a pre-work baseline, from which variations may be reckoned. Usually this will consist in having the *S* relax for a stated time before beginning the work. In certain experiments it may be more desirable to compare a normal control series with one of experimentally induced tension.

(*3*) *Practice effects.* Adaptation to the experimental situation has a notable effect on tension.[16] Thus it is frequently desirable to have several practice sessions before beginning the experimental series and to counterbalance any variations in procedure which may have a significant trend on the results.

(*4*) *Choice of performances to be studied.* Those performances which are chosen for study should be representative. It may well be that the type of performance under survey has a great deal to do with the question of whether a given amount of

[16] This statement is made upon the basis of the report that tension is reduced by practice in learning nonsense syllables (G. L. Freeman, The spread of neuromuscular activity during mental work, *J. Gen. Psychol.*, 5, 1931, 479-494).

tension will facilitate or inhibit. A desirable feature of experimentation would be the inclusion of several distinctly different tasks.

General Plan of Experimentation

The experimental program covered by the present report includes 6 separate researches, as follows: (I) Relationship between anticipation of task difficulty and tension in a remote muscle group. (II) Relationship between degree of motivation, work output, and tension. (III) Relationship between degree of muscular preparation and reaction-time. (IV) Effect of equivalent muscular reënforcement in several types of performance. (V) Effect of experimentally induced tension and relaxation upon reaction to two intensities of electric shock. (VI) Localization of tension producing optimal facilitation in a given task. Each of the above topics will be discussed separately. These discussions will be preceded by a brief description of the apparatus common to most of them and followed by an integrative résumé of the total program.

Apparatus and General Methods

Tension of the right quadriceps muscle was photographically recorded by the method of tendon deformation, previously described. This device was, in essential, a lever serving to depress the patellar tendon, connected with an optical system which magnified changes in tendon deformation (unless otherwise stated) approximately 500 times. The *S* sat in a comfortable morris chair (See Fig. 1), with a screen shielding the recording camera and attachments from his view. The lever serving to depress the patellar tendon was at first placed directly in contact with a point tattoed on the skin. In certain of the later experiments where comparable records were not at such a premium, the depressor rested over the pant-leg. In experiments where great care was taken to obtain equivalent magnification on successive days, the micrometer adjustment at the base of the chair proved invaluable. After the *S* was seated and the tendon depressor set in place, the chair, and *S*, could be moved forward or backward until the reflection of a mirror placed on the tendon apparatus coincided with a standard distance mark on the camera slit. The standard procedure in all experiments was to place the *S* in the apparatus for several sessions prior to actually beginning the experiment. This enabled the *S*s (who were with a few exceptions hired undergraduates) to become well adapted to the general experimental situation before beginning the regular series. The prework relaxation periods varied in individual experiments from 3–15 min. Occasional comments were taken from the *S* and sometimes questions were asked. The author (*F*) frequently served as an *S*, largely for the purpose of ascertaining the subjective side of the experiment. The results which are discussed are almost wholly objective and were obtained from the photographic records. In the first experiments only one *S* was used each day for fear of changing the setting of the tension recorder. Later it was found that the apparatus could be reset without loss of accuracy, thus enabling *E* to use several *S*s a day. The *S*s were chosen with some

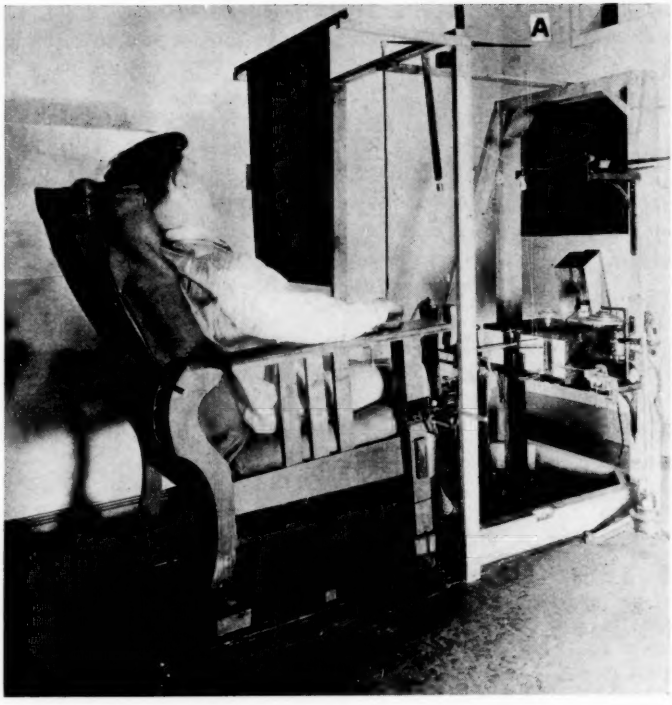


FIG. 1. APPARATUS AND EXPERIMENTAL SITUATION

care, both with respect to their reliability and the position of their patellar tendons.[10] While the method outlined does not lend itself to the rapid accumulation of data, its accuracy serves to counterbalance that disadvantage.

EXPERIMENT I

Problem. When we say that the effort of an *S* varies, we imply that some internal factor produces increments and decrements in his output. Mental effort is an ill-defined rubric covering variations in the *S*'s estimation of task difficulty, and in his motivation. Both factors are presumptively

[10] That is, position relative to the skin surface. If the tendon is imbedded and surrounded by a great deal of fatty tissue, changes in its position produced by quadriceps tension will not be transmitted to the tendon depressor. We favored, therefore, *S*s whose tendons lay freely exposed and close to the surface of the skin.

reflected in the tonus accompaniments of his mental work. To what extent does the anticipation of task difficulty influence the general tension of the organism? What effect has the degree of preparation on the efficiency of the performance? These questions were made the subject of the first investigation.

Procedure. Six *Ss* were given 60 practice trials in reacting to the slow movement of a small dot in the visual field. The reaction took the form of a compensation, *S* pulling or pushing a stick in the direction opposite to the seen movement. The dot moved at three different rates of speed, making the reaction-problem of various degrees of difficulty.[17] All the *Ss* worked under a reward incentive, calculated to produce maximal effort. Three specific variations in instruction were used, following the initial practice. There was a 15 min. rest period prior to each day's work and a 1 min. rest period between successive trials. Ten trials of each degree of task-difficulty (30 trials in all) were given in haphazard order in each of the variations. Practice effects occurring during the experimental series were equilibrated by counterbalancing the order of presenting the variations to the *Ss*. The variations were:

(*a*) *Without knowledge of rate or direction.* Although the *Ss* realized—as the result of the practice series—that the problems were of unequal difficulty, no intimation was given on which particular rate of movement would next occur. The instructions were:

Your problem for today will be to react to the movement of one of the bright dots which you see before you. Your eyes will be closed before every trial. When the 'ready' signal is given open your eyes and fixate the dots. If the right hand dot moves upward, give the stick in your hand a slight downward pull. If the dot moves down, push up with the stick. Always make an effort to react as quickly as possible; but do not sacrifice accuracy since it will count against you. If you move in the wrong direction, correct your error at once.

(b) With knowledge of rate. Just prior to the ready signal, the *Ss* were told what rate of movement would next be used. They were asked to try to increase their speed of reaction to the slower rates by being more adequately 'set.' The actual instructions were:

Your records show that you react less quickly to the slower movements of the dots. In these tests the experimenter will help you to increase the speed of your reaction. Before every ready signal he will tell you the difficulty of the problem so that you may be adequately set. 'One' will signify the hardest problem of all, 'two' the next, and 'three' the easiest. The experimenter will not tell you in which direction the dot will move.

(*c*) *With knowledge of rate and direction.* The *Ss* were informed prior to stimulation not only of the rate but also the direction of movement. This lessened the anticipated difficulty of all problems. The instructions were:

---

[17] *S* actually saw two dots: one fixed, and one movable. The pair were shown to prevent reaction to the autokinetic shifting of a single dot. Task-difficulty was a function of the rate of movement of the variable dot. The most difficult reaction (1) was in response to the slowest movement, almost imperceptible. The next most difficult reaction (2) was to a slightly more rapid movement and the easiest reaction (3) was to a readily distinguishable movement.

It is possible to further increase the speed of your reaction. The experimenter will now state not only the rate but also the direction in which the dot will move: '1 up' will signify the slowest upward movement. Be on the alert for the ready signal as usual, but do not react until you actually see the dot move. Reactions made before the movement will be counted as false. Remember that accuracy counts as much in this as in previous tests.

In order to compare these results with the tonus accompaniments of tasks utilizing another sensory avenue, two *S*s repeated variations *a* and *b* with reactions involving tactual discrimination. The problem given was to compensate for the felt movement of the stick (previously used in reacting to seen movement). Three rates of movement paralleling those in the visual problem were produced by releasing appropriate weights attached to lever A in the system pictured in Fig. 1.

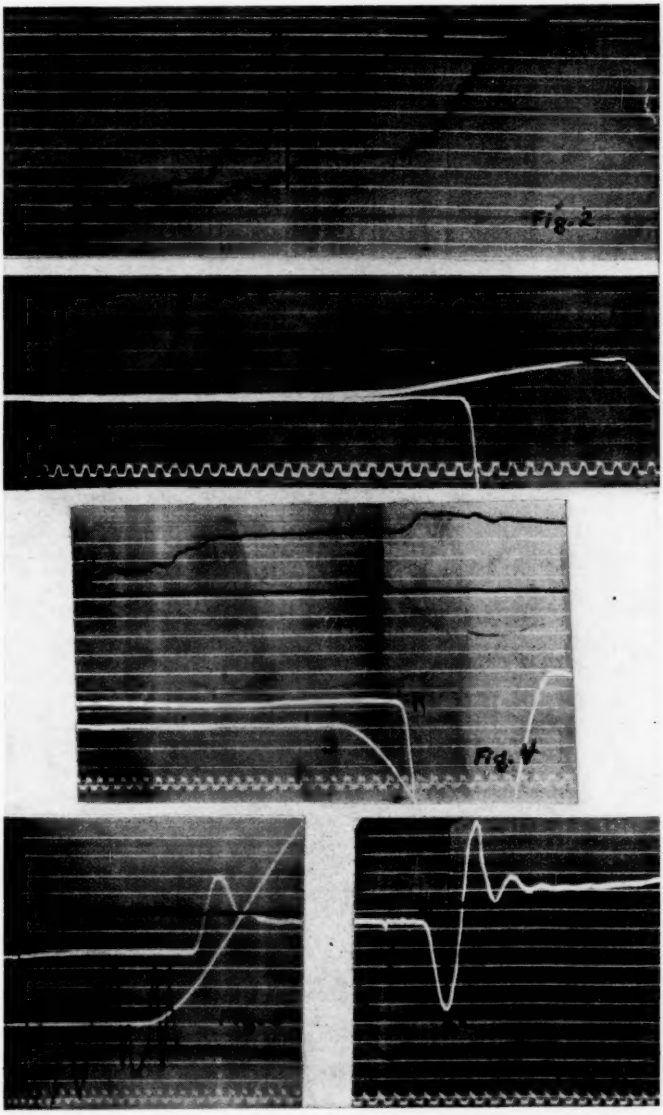
Tonus changes were photographically recorded from the patellar tendon as already described. Since anatomical differences between *S*s made it impossible to to make absolute comparisons of tonus change in terms of amount of tendon deformation (as expressed in mm. of deflection on the record), we sought a means of reducing these readings to a common denominator. The way was open through our discovery of the relationship between tendon deformation and muscular pull.[18] An apparatus was used to calibrate the photographic excursions of each *S*'s tendon record. The stirrup carrying the tendon depressor swung on an axis by finely machined cone bearings. After the stirrup and tendon depressor were satisfactorily adjusted, the lower limb hung limply from its support at the knee. The stirrup was then connected to another lever which could be raised by minimal contractions of the *S*'s quadriceps muscle. (The actual displacement of the foot was about ¼ in.) A mirror at the axis of this lever served to record the height to which the lever was lifted by the quadriceps. The excursions of the tendon depressor were photographed concomitantly with the changes in the height of the weight lever. We had already obtained by empirical means a record of the number of grams of weight which were necessary to lift the lever to various heights. By comparing a *S*'s calibration with this standard, we were able to express mm. of tendon deflection in terms of grm. tension. This system accurately measured increments in muscular tension of the magnitude of one grm. The calibration had to be made only once for each *S*, since care was taken in placing the tendon depressor exactly on successive trials.

Results. Before presenting the quantitative data, certain typical records may be profitably surveyed.

Fig. 2 reproduces the calibration record of one *S*. It will be recalled that as the *S* pushes the weight lever upwards, its excursions and the changes in quadriceps tension are simultaneously recorded. Having previously ascertained the points to which the weight lever rises under known pressures (W) it is easy to translate mm. of change in the tendon record (T) into grams tension. Thus an excursion of 10 mm. equals 1 grm. tension while an excursion of 35 mm. equals 10 grm.

Figs. 3 and 4 reproduce records taken respectively during the slow and fast rates of movement in the visual problem. The stimulus and reaction (shadow) lines are easily recognizable. The variable dark lines indicate two degrees of magnification of tendon deformation, and were produced by connecting the tendon

[18] Freeman, *op. cit.*, this JOURNAL.



FIGS. 2–6. SAMPLE RECORDS MADE IN EXPERIMENT I

depressor to cylinders of different diameter. Since the ratio between the two cylinders is known, we may accurately estimate tonus changes according to the more sensitive recorder in case the reflection therefrom leaves the photographic paper. The straight dark line is a reflection from a mirror placed upon the carrying stirrup and indicates that no leg displacement occurred during these records.

Fig. 5 shows the artifacts produced in the tendon record (T) by postural shifts. Such records are eliminated from the quantitative computations.

Fig. 6 reproduces a record taking during tactual discrimination. The point in the shadow line marked *s* is where the weight was dropped. We see that the subject compensates in the wrong direction at R^1 and corrects his error at R^2.

Quantitative. Inspection of Tables I, II, and III show that the anticipation of task difficulty is reflected in the tonus accompaniments of the

TABLE I

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Variation *a*: without prior knowledge of rate or direction)

S	Time order	Task-difficulty	Av. tonus changes in grm. pre-reaction	Av. tonus changes in grm. post-reaction
		1	1.4	1.6
Pe	a, b, c	2	1.2	1.7
		3	2.0	1.9
		1	2.4	.8
Th	a, b, c	2	5.3	1.8
		3	4.5	3.3
		1	.8	1.0
Su	b, c, a	2	1.6	.8
		3	1.4	.4
		1	.31	1.50
Li	b, c, a	2	.17	.78
		3	.33	.55
		1	1.6	.58
Se	c, a, b	2	1.5	1.00
		3	1.5	.97
		1	2.6	.33
Or	c, a, b	2	2.7	1.60
		3	1.9	2.75
		1	1.52	.97
Av.		2	2.08	1.28
		3	1.94	1.65

work. There are no consistent differences in the amount of tension which accompanies the solution of tasks of various degrees of difficulty, when the *S* is previously unaware of what that difficulty will be (Table I). There are, however, consistently greater tonus changes accompanying the hardest problem when the *S* is notified that it is coming (Table II). Knowledge

TABLE II

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Variation *b*: with prior knowledge of rate)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Pe	a, b, c	1	5 45	4.00
		2	3.36	2.65
		3	1.05	2.40
Th	a, b, c	1	5.5	.33
		2	3.8	2.60
		3	4.0	2.80
Su	b, c, a	1	1.65	1.80
		2	.80	1.30
		3	.65	1.35
Li	b, c, a	1	1.1	.40
		2	.2	.10
		3	1.2	1.06
Se	c, a, b	1	4.3	.80
		2	.5	1.75
		3	.3	.80
Or	c, a, b	1	3.2	1.60
		2	1.9	.85
		3	2.3	.38
Av.		1	3.53	1.49
		2	1.76	1.54
		3	1.58	1.47

TABLE III

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Variation *c*: with prior knowledge of rate and direction)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
Pe	a, b, c	1	2.15	1.25
		2	1.9	.05
		3	1.1	1.26
Th	a, b, c	1	.8	2.4
		2	1.8	1.0
		3	1.6	.8
Su	b, c, a	1	.7	.2
		2	.2	.5
		3	.35	1.3
Li	b, c, a	1	.31	.1
		2	.5	.5
		3	.3	.1
Se	c, a, b	1	.85	.1
		2	.1	.8
		3	.4	.1
Or	c, a, b	1	.62	.1
		2	.4	.85
		3	.75	.2
Av.		1	.90	.69
		2	.81	.61
		3	.75	.62

of the entire nature of the problem prior to its solution is reflected by the lessened tonus changes accompanying preparation and solution (Table III). The observations made with the visual problems (Tables I, II, III) are substantiated with tactual problems (Tables IV and V). All tonus changes recorded are expressed in grams tension, over the pre-work base-

TABLE IV

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Tactual problems: without prior knowledge of difficulty)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
		1	1.2	0.3
Li	a, b	2	1.5	1.6
		3	0.7	1.1
		1	1.3	3.0
Su	b, a	2	3.3	1.1
		3	2.0	0.8
		1	1.2	1.6
Av.		2	2.4	1.3
		3	1.3	0.9

TABLE V

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND TASK-DIFFICULTY
(Tactual problems: with prior knowledge of degree of difficulty)

S	Time order	Task-difficulty	Av. tonus changes in grm.	
			pre-reaction	post-reaction
		1	5.0	0.8
Li	a, b	2	1.5	1.6
		3	0.9	0.6
		1	4.9	5.1
Su	b, a	2	2.7	1.0
		3	2.1	0.7
		1	4.9	2.9
Av.		2	2.1	1.3
		3	1.5	0.7

line. The tonus changes recorded in all tables are the averages of 10 trials for each task-difficulty. The pre-reaction changes are reckoned between the ready signal and the time the stimulus moves (20 sec.). The post-reaction readings are taken between reaction and a point 20 sec. after this.

What effect has the degree of preparation upon the efficiency of the performance? This is an important question which will be considered in greater detail in connection with a later experiment. Certain indications are

apparent from the present data. Let us consider the tonus changes occurring under a given instruction an index of this preparation. Table VI compares the reaction-times and tonus changes obtaining under variations (*a*) and (*b*). We see that the increased tonus changes of (*b*) are correlated with increases in efficiency (as measured by decreases in the reaction-time.) The records for variation (*c*) are excluded from this comparison, because the difficulty of all the reactions is experimentally reduced.

TABLE VI

THE RELATIONSHIP BETWEEN TONUS FLUCTUATIONS AND REACTION-TIMES

			Variation (*a*)			Variation (*b*)		
S	Time order	Task Diff.	Av. R. in sec.	Tonus		Av. R. in sec.	Tonus	
				pre-r.	post-r.		pre-r.	post-r.
		1	.63	1.4	1.6	.41	5.4	4.0
Pe	a, b, c	2	.40	1.2	1.7	.50	3.3	2.6
		3	.40	2.0	1.9	.52	1.0	2.4
		Av.	.48	1.5	1.7	.48	3.3	3.0
		1	.78	2.4	0.8	.67	5.5	0.3
Th	a, b, c	2	.51	5.3	1.8	.58	3.8	2.6
		3	.49	4.5	3.3	.45	4.0	2.8
		Av.	.59	4.7	1.9	.57	4.4	1.9
		1	1.01	0.8	1.0	.65	1.6	1.8
Su	b, c, a	2	.71	1.6	0.8	.54	0.8	1.3
		3	.70	1.4	0.4	.64	0.6	1.3
		Av.	.81	1.3	0.7	.61	1.0	1.4
		1	.71	0.31	1.5	.59	1.1	0.4
Li	b, c, a	2	.49	0.17	0.7	.50	0.2	0.1
		3	.45	0.33	0.5	.43	1.2	1.0
		Av.	.55	0.27	0.9	.51	0.8	0.5
		1	.84	1.6	0.5	.46	4.3	0.8
Se	c, a, b	2	.55	1.5	1.0	.40	0.5	1.7
		3	.48	1.5	0.9	.43	0.3	0.8
		Av.	.62	1.53	0.8	.43	1.7	1.1
		1	.72	2.6	0.3	.48	3.2	1.6
Or	c, a, b	2	.69	2.7	1.6	.53	1.9	0.8
		3	.53	1.9	2.7	.50	2.3	0.3
		Av.	.65	2.4	1.5	.50	2.5	0.9
		1	.78	1.5	0.9	.54	3.5	1.4
Av.		2	.56	2.0	1.2	.51	1.6	1.5
		3	.51	1.9	1.5	.49	1.5	1.4
		Av.	.62	1.8	1.3	.51	2.3	1.5

Discussion. Knowledge of task difficulty influenced the degree of subjective 'effort.' Records of muscular tension reflected these variations. Experimental reduction of task difficulty decreased tonus changes. Increased tension due to anticipation of task difficulty and increases in efficiency of reaction are apparently positively related. It will be recalled that our results are in opposition to the report of Golla and Antonovitch that as the task became "progressively more difficult, the period of heightened tension was not prolonged." These authors, however, were more concerned with the form of his tonus curves than with quantitative values. Our chief criticism of their work rests upon the fact that they gave no indication of how it was determined that their problems actually became more difficult. This same criticism can be applied to a lesser degree in the work Bills, who reports that the tension of adults increased when they were running the more difficult parts of a maze. In neither of Golla and Antonovitch's nor Bills' research has task-difficulty been equated upon as objective a basis as our own.

Experiment II

Problem. We have seen that the work of Duffy with children indicates that high tension is sometimes detrimental to precise performance. The work of Bills on the other hand suggests that tension might be increased indefinitely without having an inhibitory effect. The problem of the following investigation was to seek evidence of both facilitative and inhibitory effects in the same task. Our plan was to elicit variations in the motivation of subjects and to observe the changes which such variations might produce in output and tension.

Procedure. Eight *S*s were selected for competitive work in continuous addition. They were divided into two groups, each *S* working for a prize. Two experimental sessions were held for every *S*. On the first day, all added columns of figures for three 5-min. periods. On the second day they worked 5 min. under normal maximal incentive and 5 min. each under incentives calculated to produce 'super' and 'sub-maximal' effort. The instructions were:

> You will be given tests in addition for two consecutive days. After you are comfortably seated in the apparatus try to rest and relax. After a time I will give you the addition sheets and tell you to begin work. Enter upon these tests with maximal effort, as a prize of $5 will be given to the speediest and most accurate adder in your group. By the second day I will know something of what the others have done, and will so advise you. Thus you may know how you are measuring up with the rest. Sit quietly during the tests. Put down your totals only. Whenever I announce the end of a 5-min. interval place a check mark upon the column of figures you are adding.

The order of presenting the experimental variables was properly equilibrated. Sub-maximal effort was sought by telling the *S* there was no chance for him to win the prize, his yesterday score being found to be so far below those of his

competitors. Super maximal effort was sought by informing the *S* that there was, after all, a chance of his winning the prize by extra effort; this was reënforced by standing over him for the next 5 min. and 'dogging' him on.

Results. Quadriceps tonus was recorded as in Experiment I, except that no attempt was made to convert the changes in mm. of deflection to grams tension. Thus the actual values tabulated are not rendered significant; that is, changes in the deflection values for the different *S*s do not represent equal changes in their quadriceps tonus.

TABLE VII

PERFORMANCE AND TONUS
(1st day: control series)

S	1st test period		2nd test period		3rd test period	
	Perf.*	Mean Tonus	Perf.*	Mean Tonus	Perf.*	Mean Tonus
Ci	20/2	13.0	32/2	63.0	21/5	32.0
Wi	20/2	28.0	20/4	25.0	20/2	21.0
Dr	15/2	32.5	30/4	72.7	22/4	67.5
Ma	15/2	9.5	18/2	13.0	17/1	11.0
Fe	14/3	22.0	15/5	17.5	14/2	18.5
Fo	10/2	34.0	12/2	69.0	10/3	42.0
Fa	10/0	7.1	12/0	28.4	12/2	42.0
La	15/2	50.5	14/5	19.0	15/2	8.1
Av.	15/1.9	26.1	19/3	38.4	16/2.6	30.3

* Performance scores include number of columns added in 5 min. over the number of columns wrong.

Discussion. The tables indicate that while tension is a factor normally contributing to the facilitation of mental work, it may become an inhibitor of precise performance. Tension was notably increased under the conditions of 'supermaximal' effort; and concomittently, the speed of addition

TABLE VIII

PERFORMANCE AND TONUS
(2nd day: under various incentives)

S	1st test period (max. incentive)		2nd test period (sub-max. incentive)		3rd test period (super-max. incentive)	
	Perf.	Mean Tonus	Perf.	Mean Tonus	Perf.	Mean Tonus
Ci	34/8	23.0	29/4	10.0	46/20	45.1
Wi	19/4	16.5	17/3	8.4	21/5	71.0
Dr	25/6	6.8	17/4	11.5	24/3	28.5
Ma	19/3	2.1	15/5	1.8	17/7	17.6
Fe	15/2	7.3	17/5*	12.5	15/4	11.7
Fo	15/2	44.0	9/4	50.0	16/8	71.0
Fa	18/4	51.7	18/5*	65.2	17/3	102.5
La	18/6	3.0	14/6	40.5	20/13	89.2
Av.	20/4.4	19.3	17/4.5	24.9	22/7.9	54.6

increased and accuracy decreased. The occasional instructions which sought to secure 'sub-maximal' effort were effective in all but two cases (*Fe* and *Fa*). These two *Ss* apparently took exception to the suggestion that they could not win and so redoubled their efforts. Table VIII shows that their mean tension increased and performance improved during this period rather than the reverse.

Experiment III

Problem. One of the more traditional approaches to the problem of facilitation and inhibition in performance has been through the voluntary reaction-time. The presentation of stimuli to two sense modalities may decrease or lengthen the voluntary reaction to one of them, depending upon time relations of the stimuli, (Jenkins).[19] The following experiment studied the effects of increased muscular tension upon reaction-time to sound. Since it is known that reaction-time is normally dependent upon the strength of the stimulus, the experiment took the form of comparing reaction to two intensities of sound under conditions (*a*) of greater and (*b*) of less tension. No precise means for defining the amount of tension or the time of its appearance were possible. By instruction, however, the *S* took the tension 'set' at every ready signal (2 sec. before the stimulus was sounded) and the photographic records of quadriceps tension gave some indication of the amount of neuro-muscular 'spread.'

Apparatus and technique. The stimuli used were clicks of the diaphragm of the *S*'s earphones. The first (which will hereafter be designated as 'weak') was slightly above the R.L. of the 4 male *Ss* used. The second ('loud') was well above the limen, but not too intense to cause discomforture when heard through the earphone. These clicks were produced by breaking an electric circuit through a relay, outside the soundproof room in which the experiments were held. By means of a double-pole, double-throw switch, the current could be made to pass through either of two inductoriums, with their secondary coils appropriately set for 'weak' and 'loud' clicks.

S sat in a morris chair. He was blindfolded and rubber 'ear-muffs' were placed over the earphones to eliminate secondary cues. Photographic records of reaction-time and right quadriceps tension were taken after the usual manner. A neon tube provided the time lines of the record. The latency of the Becker markers, used in the stimulus and reaction-lines, was found to be from 7 to 10 sigma. No correction has been made for such errors, however, in the reading of the records.

Procedure. All the *Ss* received 100 practice trials at the first experimental session. Thereafter they were divided into two groups. Group 1 worked for 4 consecutive days under instruction (*a*), and then for 4 days under instruction (*b*). Group 2 reversed the time-order of Group 1. The experimental variations were as follows:

[19] T. Jenkins, Facilitation and inhibition, *Arch. Psychol.*, 14, 1926, 1-55.

(*a*) *Control series.* Fifty reactions each to 'weak' and 'loud' stimuli were taken at every experimental session. The stimuli were presented in haphazard order with the following instruction:

Your problem is to react to these auditory stimuli as quickly as possible. When you hear the ready signal, press your finger on the reaction-key. Release the key immediately you hear the click in the earphone. It is important that you sit quietly during these tests. An interval will elapse between successive trials, during which you will count numbers to yourself. Try not to think of the next reaction until you hear the ready signal.

(*b*) *Tension series.* The same number of reactions (50) were taken as in (*a*). While 'weak' and 'loud' stimuli were still given haphazardly, *S* was informed at every 'ready' signal whether the following click would be weak or loud. His particular problem was to concentrate on reaction to the weak stimuli, attempting to shorten his time by being 'tense' and 'set.' The instructions were:

Your problem is to react as quickly as possible to these two intensities of auditory stimulation. It generally takes longer to react to the 'weak' than to the 'loud' stimuli. By being properly set or ready, however, you can greatly increase the speed of your reaction to the 'weak' stimuli. At each ready signal I am going to state what the next stimulus will be. Do not relax your efforts to react quickly to the 'loud,' but exert an added effort when I say 'weak.' You will find that it will help to get your muscles into a state of slight tension or readiness to react whenever you hear the signal.

Results. The logic of the experiment rests with the comparison of the reaction-time to weak and loud auditory stimulation under the two conditions (*a*) and (*b*). If reaction to 'weak' under (*b*) is notably decreased, we have evidence of facilitation by way of increased tension. If the reaction-time is increased, the data favor the opposed interpretation. Reactions taken under (*a*) provided the necessary 'standards' for comparison.

TABLE IX

AVERAGE REACTION-TIME AND TONUS INCREMENT IN CONTROL SERIES

(Time in sigma; Tension in mm.)

S	Time order	Weak stimuli				Loud stimuli			
		Reaction-time		Tension		Reaction-time		Tension	
		Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
Sm	1	222.0	18.3	6.14	2.7	194.2	15.0	7.26	3.8
Ba	1	247.1	27.1	9.81	2.8	185.6	16.6	9.76	3.1
Ho	2	183.0	16.5	4.41	1.8	152.9	23.0	4.93	2.1
Fe	2	163.5	23.0	4.72	2.2	119.2	16.4	6.80	2.8
Av.		203.9	21.4	6.72	2.4	162.9	17.7	7.19	2.9

In Table IX are given the average reaction-times and tension scores obtained under condition (*a*). Table X gives similar data obtained under (*b*). The reaction-time scores are in sigma and the tension scores are in mm. of deflection (from a baseline established prior to the ready signal).

These tension scores represent the average height of increment per record when the changes in tendon deformation are magnified approximately 800 times.

TABLE X

AVERAGE REACTION-TIME AND TONUS INCREMENT IN TENSION SERIES
(Time in sigma; Tension in mm.)

S	Time order	Weak stimuli				Loud stimuli			
		Reaction-time		Tension		Reaction-time		Tension	
		Av.	m.v.	Av.	m.v.	Av.	m.v.	Av.	m.v.
Sm	1	152.4	24.8	11.07	4.7	150.7	12.5	7.03	2.9
Ba	1	148.9	16.8	4.93	2.3	144.4	21.8	2.71	0.9
Ho	2	152.6	20.4	5.21	2.6	148.9	18.7	4.68	2.1
Fe	2	150.8	29.8	15.70	4.7	127.0	37.0	10.23	3.5
Av.		151.2	22.9	9.23	3.8	142.7	22.5	6.16	2.4

Discussion. The tables show that reaction-time to 'weak' stimuli in the tension series (*b*) was appreciably shorter than in the control series (*a*). This holds even for *Ho* and *Fe,* who reacted first under (*b*). (We might expect that practice would have reduced their reaction to 'weak' stimuli in (*a*), since a comparison of reaction-times to the loud stimuli shows such a reduction on the last 4 days of the experiment.)

That the *S* accepted instruction (*b*), is indicated by their tension scores, which are greater for 'weak,' than for 'loud' stimuli. They reported that the 'set' for 'weak' stimuli tended to carry over into their preparation for the 'loud' stimuli. We may conclude that the data of this experiment indicate that muscular tension (in the form of a preparatory set) may facilitate reaction-time to sound.[20]

EXPERIMENT IV

Problem. Previous work has shown that the normal tonus accompaniments vary with the type of performance under survey.[21] The problem of this investigation was to study the effects of approximately the same amount of tension upon (a) finger oscillation and (b) manual pursuit. The *S*s used in the previous experiment showed lower tension scores for manual pursuit than for finger oscillation. Will increments in tension be equally facilitative in both performances?

[20] We suggest further that the shortening of reaction-time under the 'traditional motor set' may be due to the presence of greater tension under this condition. This seems to be bourne out by the work of Judd (*Psychol, Monog.,* 7, 1905, 199-226) and Williams (*Psychol. Monog.,* 17, 1917, 55-155).

[21] Cf. Freeman, The compensatory effects of muscular tension subsequent to sleep loss, *J. Exper. Psychol.,* 15, 1932, 267-283.

Procedure. Two *Ss* who had received extensive practice in both performances (in connection with the other experiment), undertook a series of trials in which the amount of tension was under a certain degree of experimental control. Tests were of 30-sec. duration and were separated by a 5-sec. interval. Fifteen trials of each performance were completed on each of 5 experimental days. The time-order was properly counterbalanced. Photographic records of performance and tension were obtained after the usual procedure.

The experimental variation of tension was achieved by having *S* sustain different weights with the left arm while reacting with the right. The weights acted over a pulley and *S* was not allowed to support his elbow in sustaining them. A record of the tonus in a remote muscle group (quadriceps) indicated the 'spread' of the experimentally induced tension. Five trials of each performance were taken daily under (1) control, no induced tension, (2) arm sustaining 5 lb. weight, (3) arm sustaining 12 lb. weight. The *S* attempted maximal effort under all conditions and in so far as possible, sought to ignore any discomforture connected with holding the weights. That this occasionally occupied their attention was indicated by their reports.

Results. Figs. 7 and 8 reproduce typical records of finger oscillation taken with the left arm sustaining the 5 and 12 lb. weights respectively. Figs. 9 and 10 reproduce records of manual pursuit taken respectively under no induced tension and with the 12 lb. weight sustained. In the records, quadriceps tonus is noted as 'Q' and the line 'C' indicates that no movement of the right leg has occurred during the observation; in Figs. 7, 8, and 10, the movement of line 'W' indicates the variability in the *Ss* attempt to hold the weight steady. The reader will note the tendency for manual pursuit to be less accurate under the condition of greater tension, whereas the speed of finger oscillation suffers no reduction.

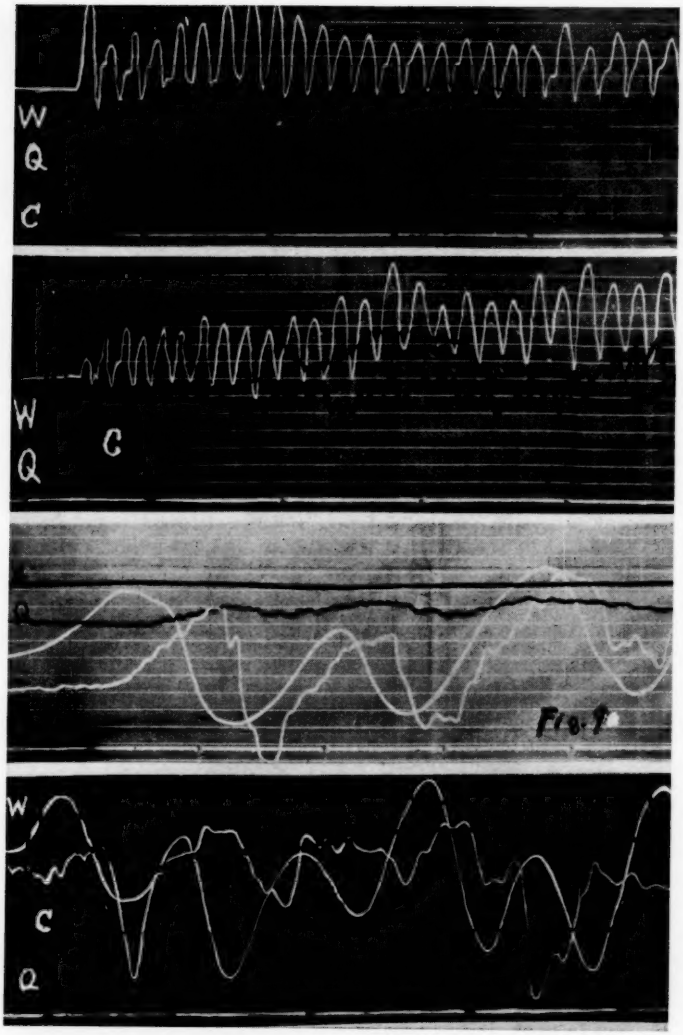
Table XI gives the average tonus and performance scores in the 3 tests of finger oscillation. Table X shows the average tonus and performance scores obtained in the test of manual pursuit. In both tables the tonus scores represent the average increment in mm. of deflection per sec. The performance scores are, respectively, the number of finger oscillations per sec. and the average accuracy of pursuit per sec. The method of determining these scores was the same as described in a previous communication.[22]

TABLE XI

QUADRICEPS TONUS AND FINGER OSCILLATION SCORES UNDER THREE CONDITIONS OF TENSION

S	No induced tension		5 lb. weight		12 lb. weight	
	Perf.	Quad. Tonus	Perf.	Quad. Tonus	Perf.	Quad. Tonus
Sm	5.5	7.2	5.4	10.8	5.7	12.5
Fe	5.7	15.3	5.9	19.8	5.7	22.2

[22] *Loc. cit.*



FIGS. 7–10. SAMPLE RECORDS MADE IN EXPERIMENT IV

TABLE XII

QUADRICEPS TONUS AND MANUAL PURSUIT SCORES UNDER THREE CONDITIONS OF TENSION

S	No induced tension		5 lb. weight		12 lb. weight	
	Perf.	Quad. tonus	Perf.	Quad. tonus	Perf.	Quad. tonus
Sm	.11	2.7	.18	5.8	.29	12.6
Fe	.15	4.5	.16	4.3	.24	15.7

Discussion. The indication from this rather limited data is that the facilitative effect of muscular tension on higher functions is dependent upon the type of performance under survey. The work output of a simple task (such as finger oscillation) may even increase in proportion to the amount of recorded tension. In any case, it suffers little or no retardation when the amount of tension is increased experimentally. Contrawise, the accuracy of manual pursuit decreases with the introduction of experimental tension. In this latter case, we have a performance which requires a high degree of sensori-motor coördination. It might be argued therefore, that the presence of excess tension tends to 'fog' or to disturb the finely integrated act. We thus begin to arrive at an explanation of Johnson's report that the reactor maintaining slight tension is most proficient in coördinated hand movements.[23] Additional work upon this problem is highly desirable.

EXPERIMENT V

Problem. This experiment sought to determine the effect of varying degrees of experimentally induced tension upon reactions to electric shock. While this problem has been previously attacked,[24] none of the investigators have compared the effects of tension, normal and relaxation states, in the same experiment, nor have any objective records been taken of muscular tension during the experimentally induced states. It seemed of value, therefore, to plan the present experiment along such lines.

Apparatus. In a study of responses obtaining under a variety of muscular conditions, it is important that the stimulating agent be a constant factor. The difficulties usually encountered in using electric shocks include (1) variations in intensity during stimulation, (2) no means of varying the intensity systematically, and (3) inability to measure intensity. After the trial of several less satisfactory methods, the following device was adopted.

Alternating current of 10 volt potential was obtained from a toy transformer. This passed to the primary of an induction coil with a resistance of 12 ohms. Three extra resistances (of 50, 13 and 1 ohms, respectively) could be thrown into

[23] B. Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 329-341.

[24] Cf. E. Jacobson, *Psychol. Rev.*, 18, 1911, 24; this JOURNAL, 23, 1912, 345; 36, 1925, 73. More objective controls were employed by M. Miller, *op. cit.*

series with this current by means of appropriate switches. The secondary of the inductorium, which could be moved as much as a meter away from the primary coil, was placed in series with the *S*s and with *E*'s key. Photographic registration of the onset and cessation of stimulation were possible by use of the Dodge mirror-telephone recorder. The diaphragm of the recorder was placed across a shunt resistance in series with the secondary circuit.

The system was used as follows: With the 50 ohm resistance in series with the primary, the sensory threshold of *S* was obtained at each experimental session. During the session two intensities were used, both above this threshold. The first intensity was obtained by placing the 25 ohm resistance in series with the *S* and the second intensity was obtained by using the 1 ohm resistance; that is, the less resistance in the primary circuit the greater the current passing through *S*. Since the potential remained constant and since the secondary coil was set for threshold stimulation with R_1 in series, stimuli obtained from use of R_2 and R_3 could be defined in terms of this threshold. Polarization changes in the tissues are practically eliminated by the use of alternating current.

The *S* sat in a morris chair (as in Fig. 1) with his right wrist strapped to a moistened brass electrode and the tip of his index finger resting in a liquid electrode. A string fastened around the first joint of this finger was suitably connected with a lever arrangement for photographing the extent of finger withdrawal. The index and middle fingers of the left hand rested lightly over two Lindley rotary switches used for discrimination reactions. These switches were connected with the Dodge-Travis duplex-marker, arranged for photographic registration. Right quadriceps tension was obtained after the usual manner, a three mirror system being used to make adjustment unnecessary during the session.

Procedure. The experiment was designed to measure (1) non-voluntary or 'reflex' reaction to electric shock, (2) extent of finger withdrawal, (3) discrimination time, and (4) accuracy of discrimination under different conditions of tension. These conditions were (*a*) relaxation, (*b*) hyper-tension, and (*c*) normal, *i.e.* the control. The 9 *S*s were divided into 3 groups, each group completing the experimental variations in a different time order. A 5-min. rest period at the beginning of each experimental session was used to establish the pre-work (tonus) baseline. The discrimination reaction (left hand) was introduced primarily to detract the *S*'s attention from the hand receiving the shock. This helped to make finger withdrawal automatic. The discrimination problem was a paired comparison; the first stimulus was always weak; and the second, which followed after a one second interval, was either the same or greater. Twenty reaction pairs each were given under conditions *a, b,* and *c*.

(*a*) *Relaxation.* A relaxed state is most difficult to achieve with unpracticed *S*s. In the first place the strangeness of the apparatus and situation is likely to result in an increase of tension. In the second place, it is hard to keep relaxed when there is expectation of shock. Preliminary explorations, however, showed ways of successfully handling this experimental variable. All *S*s were practiced in the method of progressive relaxation for several sessions prior to the major experiment.[25] Re-

[25] The *S* contracts the various muscle groups to obtain the experience of tension, then practices in the elimination of such experience. Cf. E. Jacobson, The technique of progressive relaxation, *J. Nerv. & Ment. Dis.*, 53, 1921, 283.

laxation was favored further by arranging the chair back so that the *S* rested as on a bed. The right leg was stabalized and carried the tendon depressor as usual. All apparatus was screened from view and the room completely darkened. The ticking of a clock and the rhythmical hum of a motor served as suggestion devices, while at the same time masking any slight noise incidental to operating the camera. Pairs of shocks were given every 3 min., an interval which inspection had shown to be sufficiently long (1) to eliminate any sensory (adaptation) effect of the previous shock and (2) to permit the course of relaxation to be relatively undisturbed. The instructions were:

After you are adjusted in the apparatus, begin to relax. During rather long intervals in the hour, you will feel a pair of electric shocks delivered to your right hand. Do not let these disturb the progress of relaxation. They are quite harmless and will cease as soon as you lift your finger. This you will do automatically, so do not attempt to attend to it. However, as long as you remember, try to discriminate between the intensity of the two shocks. Use the other hand for these reactions. The first shock given will always be the standard and you will strike the first key for this. Then second, which follows almost immediately will either be of the same or greater intensity. If the same strike the first key again. If greater, strike the second key.

(*b*) *Hyper-tension.* A familiar expedient in producing tension experimentally is to have *S* grip a dynamometer or sustain a weight. Previous experiments indicated, however, that such tasks as gripping the dynamometer frequently tend to become the major problem, to the relative neglect of the performance under observation. In the present research a means was found of producing tension which (as a problem) entered into the organization of the focal behavior pattern (reaction to shock) rather than interferred with it. The back rest of the chair was removed and the *S* was made to rest his chest against a board, so arranged that it placed him slightly off-balance. His arms were clamped into position on the reaction boards. Thus in order not to cause serious discomforture to his arms, *S* was forced to sit up against the chest-board. This in turn threw all the anti-gravity muscles of the trunk and neck into a state of heightened tension. The problem and general instructions were the same as used in the relaxed state.

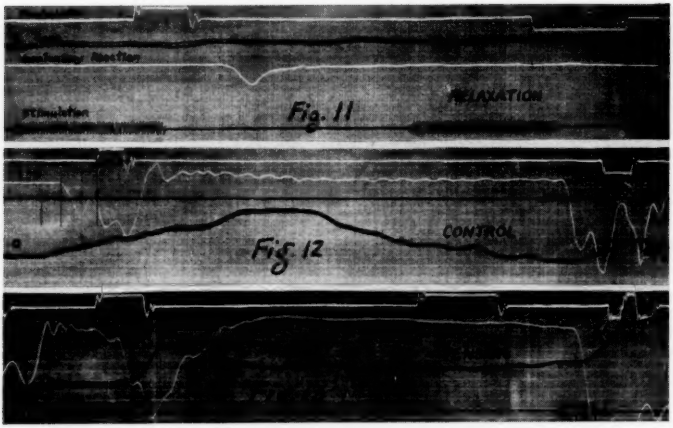
(*c*) *Normal tension: control series.* The *S*s were seated comfortably, with back and head supported. They were instructed to remain alert to the problem, making their discriminations as rapid and as accurate as possible. The same general situation held as in (*a*) and (*b*).

The records of quadriceps tonus during (*a*), (*b*), and (*c*) gave some indication of the general neuro-muscular state of the *S*. The question arises, however, as to whether these records are comparable. This is presumptively true for records were obtained under (*b*) and (*c*), with *S* erect; but the situation in (*a*) produced a different postural adjustment. The work on the physiology of posture has shown that the muscles in question are affected by the position of head and trunk (Walshe).[20] It is not known how much such factors influenced the records. It seems probable, however, that the spread of excitation (except such as was caused

[20] F. M. R. Walshe, The work of Magnus and his collaborators on the nervous regulation of posture, and its bearings on some modern neurological problems, *Med. Sci. Abst. Rev.*, 7, 1923, 1909-118.

by the postural shift) found similar reflection in records obtained under all the conditions.

Results. The records indicate that there was a marked difference in muscular tension (as recorded from the quadriceps) and in general behavior under the three experimental conditions. Fairly typical responses



FIGS. 11–13. SAMPLE RECORDS MADE IN EXPERIMENT V

Fig. 11 reproduces a record obtained under conditions of relaxation, *i.e.* (*a*). Quadriceps tonus shows characteristic features of the muscle at rest. It will be noted that this *S* withdrew his finger only slightly at the first stimulus and not at all at the second. In the latter case 'reflex' time cannot be reckoned. In this record, the discrimination of the second stimulus is 'wrong' since it is of the same intensity as the first. It will also be noted that the first stimulus was cut off (by *E*) before *S* withdrew his finger. This is because *E*'s cue to release his key is the sound made by *S* in the discrimination reaction. This reaction usually follows after the automatic withdrawal. During the relaxation series it was found advisable to continue stimulation slightly after hearing the discrimination reaction.

Fig. 12 reproduces a record obtained under control conditions, *i.e.* (*c*). Not only is finger withdrawal more rapid under these conditions, but the amplitude is much greater. The discrimination reaction is 'right' since in this record the 'stronger' stimulus followed the 'weaker.' The method of reading 'reflex' and discrimination times is also indicated on this record.

Fig. 13 reproduces a record obtained under conditions of hyper-tension, *i.e.* (*b*). Prior to the first stimulus, *S* has made anticipatory reactions both with the discrimination-key and with the right index finger. The extent of withdrawal is pronounced. Another anticipatory reaction occurs before the second stimulus and the discrimination of the actual shock is corrected and recorrected.

are shown in Figs. 11, 12, and 13. In these records the uppermost white line gives the *S*'s discrimination-reaction. The variable white line shows the amount of finger withdrawal upon shock. The record of quadriceps tonus is easily recognized, as is the period of electrical stimulation. The fact that the amplitude of this line varies in the records is due to differences in threshold values. The upright time lines (produced by the neon tube) are less distinguishable. The horizontal reference lines are 5 mm. apart.

The quantitative treatment of the data has been influenced by arbitrary decisions which are readily appreciated. (1) In obtaining the average individual scores, reactions to both intensities of shock have been considered together. This is permissible since the number of reactions to 'weak' and 'strong' stimuli is the same in all three experimental situations. (2) The tonus scores are the average of the mean values obtaining in each record. These values (in mm. of tendon deflection per sec.) are reckoned from the point in the pre-work baseline just prior to the first stimulation. Increments in tension are thus represented by plus signs and decrements by minuses. (3) For purposes of exposition, the withdrawal of the right finger has been designated as 'reflex' on 'non-voluntary' to distinguish it from the more deliberative discrimination-reaction. This is somewhat misleading; the average 'reflex' time values for relaxation, tension, and control series are, respectively, 401, 247, and 259 sigma—all within the range of voluntary reaction-time. They are considerably greater than the averages reported by Miller in an experiment somewhat similiar to our own.[27] The times obtained by Miller for relaxation and control series are, respectively, 148 and 162 sigma. The reason for these differences between our results of those of Miller lies presumptively in the intensity of stimuli used, our stimuli being relatively weak. Plotting the frequency distribution of reaction-times obtained in our control series showed a bimodal curve in the case of 4 *S*s. This indicates that our stimuli were not always intense enough to cause a rapid non-voluntary reaction. (4) Occasionally, no observable reaction was made under relaxation. These cases have been eliminated from the quantitative comparisons. (5) In such instances as where the *S* made a wrong reaction but subsequently corrected it, discrimination-time was reckoned in terms of the correct reaction. (6) All reactions with the right hand have been averaged together, although only the last reaction of the pair rightly denotes a 'discrimination.' A comparison of the reaction-time to the first and last of the paired stimuli showed an average latency of 18.3 sigma for the second reaction.

[27] Miller, *op. cit.*, 34.

TABLE XIII

REACTION AND TONUS SCORES DURING RELAXATION

Time order	S	Reaction-time (in σ)		Ext. withdrawal (in mm.)		Disc.-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
	Be	370	66	16.0	7.0	870	103	.7	−20.6
1	*Bl*	207	76	1.6	0.4	523	84	.6	−17.8
	Wt	454	82	1.1	0.9	624	93	.6	−71.6
	Kn	483	54	24.7	8.2	845	68	.8	−17.3
2	*Wg*	465	77	1.5	1.8	508	79	.6	−14.6
	Wo	544	72	7.2	3.0	598	73	.7	−21.9
	Br	240	85	17.1	13.0	457	94	.8	−17.3
3	*Fr*	205	72	17.7	8.0	525	106	.55	−12.3
	Or	646	102	5.1	4.0	756	86	.45	−68.8
	Av.	401	76.2	10.3	5.14	634	85.1	.65	−29.1

Time of withdrawal. A comparison of Tables XIII and XV shows that the time of the 'non-voluntary' finger withdrawal from shock is greatly lengthened under relaxation, for 7 of the 9 *S*s. It is about the same for *Bl* and less for *Br*. These scores might be due to practice effects, since inspection shows that *Br* took the relaxation-test on the third day and the control test on the second day. We are inclined, however, to attribute the deviation to the degree of relaxation present. Table XIII shows that *Br* was among the least relaxed of the *S*s.

Our differences between our relaxation and normal scores are more pronounced than Miller's. While this might be due to differences in the intensity of stimulation, we are inclined to emphasize the dissimilarity of the control procedures. It will be recalled that Miller allowed her *S*s to remain lying on the couch during the control series, with the negative instruction, "Do not be relaxed this time." In contrast, our *S*s sat erect and received the positive instruction to be 'alert and expectant.' This latter condition more nearly approximated our tension series than

TABLE XIV

REACTION AND TONUS SCORES DURING TENSION

Time order	S	Reaction-time (in σ)		Ext. withdrawal (in mm.)		Disc-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
	Br	305	95	33.1	18.0	317	63	.55	+ 59.9
1	*Fr*	164	38	40.0	10.2	207	41	.45	+ 47.4
	Or	299	52	24.0	11.0	222	56	.75	+ 66.6
	Be	146	56	43.8	16.1	336	65	.9	+ 91.8
2	*Bl*	145	40	43.7	11.0	290	22	.5	+ 76.6
	Wt	323	61	3.7	1.9	473	65	1.0	+ 17.7
	Kn	271	48	17.1	3.2	396	54	.5	+ 117.6
3	*Wg*	260	50	22.4	6.1	620	63	.9	+ 76.4
	Wo	313	64	19.6	6.2	335	68	.7	+ 30.2
	Av.	247	56	27.5	9.3	355	55	.69	+ 64.9

TABLE XV

REACTION AND TONUS SCORES IN CONTROL SERIES

Time order	S	Reaction-time (in σ)		Ext. withdrawal (in mm.)		Disc.-time (in σ)		Disc. acc. (in %)	Tonus (in mm.)
		Av.	m.v.	Av.	m.v.	Av.	m.v.		
1	*Kn*	295	57	50.7	12.1	755	68	0.9	+16.5
	Wg	264	41	17.4	3.5	454	31	0.9	+21.1
	Wo	260	53	18.2	6.0	278	47	0.9	+22.3
2	*Br*	315	65	34.2	12.0	381	63	1.0	+ 6.9
	Fr	165	30	16.1	7.2	346	65	0.9	+22.9
	Or	318	50	30.4	10.0	432	76	1.0	+ 2.8
3	*Be*	228	44	38.4	13.0	578	48	1.0	+25.8
	Bl	208	58	39.1	10.3	358	55	1.0	+26.9
	Wt	279	45	7.6	2.1	398	49	1.0	+18.2
	Av.	259	49	28.0	8.5	442	56	0.97	+18.2

our relaxation, a fact that may explain why the difference in reaction-time is so slight between the tension and control series. The average time is less under tension for 6 of the 9 *Ss*. It is about the same in both tension and control series for *Fr* and greater for *Wt* and *Wo*. Again, changes in recorded quadriceps tension indicate that *Fr, Wt,* and *Wo* were less tense in the tension series than were the other *Ss*. Compare, for example, the average tension and reaction scores for *Kn* and *Wt* under control and tension series. While *Wt* is about as tense in the control as in the tension series, *Kn*'s tension is increased. *Wt*'s reaction-time is greater in the tension series; but *Kn*'s is reduced. This observation would argue for the facilitative effect of tension.

Extent of withdrawal. The extent finger withdrawal also proves to be a valuable indicator of reactivity under the three experimental conditions. Tables XIII, XIV, and XV show that the average was 10.3 mm. under relaxation, 27.5 mm. under tension, and 28 mm. in the control series. All *Ss* except *Fr* showed a marked reduction under relaxation. While *Br, Or, Wt,* and *Kn* show less extensive movement under tension, their scores show a greater variability than was found in the control series. The very slight difference between the tension and control scores again indicates that the experimentally induced tension had relatively less effect upon the non-voluntary reaction to shock than did the state of experimentally induced relaxation.

Discrimination-reactions. If there was any doubt concerning the advisability of including this complication in the experimental situation, the end has justified the means. We have seen that the speed and amplitude of non-voluntary reaction was about the same under both tension and normal conditions. The discrimination time, however, is considerably reduced under tension. The discrimination is 97% accurate in the control series and only 69% accurate under tension. Under relaxation, the discrimination time is notably increased. Accuracy suffers as well, for only 65% were correct. The indication is that tension affects accuracy of discrimination in a manner analogous to the results of Experiment II.

Tonus changes. We have found the recorded changes in quadriceps tonus a help-

ful means for ascertaining the relative efficacy of the experimental variables in different *S*s. In fact, a comparison of the tonus changes aided materially in the interpretation of the *S*'s performance. In the control series *Br* and *Or* show very slight tonus changes. May these not be related to their low performance scores? Space does not permit the citation of further examples from the tables. Our suggestion points to the necessity for more detailed work.

Effects of progressive relaxation. Jacobson has reported the gradual diminution of conscious processes as the *S* relaxes progressively.[28] Since the recorded changes in tonus indicate that our *S*s were most relaxed at the end of the experiment, it has been decided to discuss certain differential effects apparently exerted by the neuromuscular state in the course of the session. For the sake of simplicity and to conserve space, these results are plotted in the form of composite curves. Fig. 14 shows

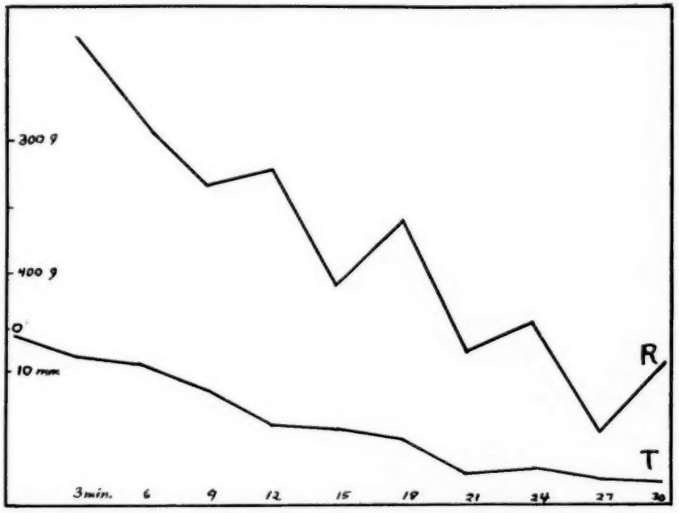


FIG. 14. COMPOSITE TENSION AND REACTION SCORES MADE DURING 30 MINUTES OF RELAXATION

the average tonus and non-voluntary reaction scores during the first 10 repetitions of the stimulus pairs. These presentations were separated by intervals of 3 min. and are represented on the abscissa of the graph. The ordinate carries both the performance scores (in sigma) and the tonus scores (in mm. of deflection). The reaction scores used in deriving the curve are to the first stimulus of the pair. Only 10 presentations are plotted, since the presence of 'non-reaction' after this point makes further compositing impossible.

[28] E. Jacobson, *op. cit.*, this JOURNAL, 36, 1925, 73-87.

Discussion. Discrimination and non-voluntary reactions to two intensities of electric shock were recorded under three situations (a) relaxation, (b) typer-tension, and (c) normal tension. Relaxation was found to lengthen the reaction-time, shorten the extent of finger movement (withdrawal), increase the time and decrease the accuracy of discrimination. Hyper-tension decreased the accuracy of discrimination, while non-voluntary reaction tended to be more rapid. Occasional tests made at the close of the experimental sessions showed that the sensory threshold underwent greater alteration during tension and relaxation than during equivalent periods in the control series. The indication was that the sensory threshold was raised under relaxation and lowered under conditions of tension. This apparently confirms the earlier report of Jacobson,[29] which provided introspective evidence that the intensity of sensations was lessened under relaxation. It is also of interest to point out that White found that there is a higher electrical resistance of the body under relaxation than under tension.[30] This factor is intimately related to threshold values, such as we measured.

A curious phenomena developed with *Wg* in the relaxation series. During the latter half of this session, this *S*, while apparently asleep and showing no non-voluntary reaction to the first intensity of stimulation, continued to perform the discrimination-reaction with the other hand. Questioning of this *S* indicated that he had adopted some sort of 'set' to react to the interval. That such activity can be carried out upon a very low level of reactivity is seen by the fact that one can occasionally 'drive' a car in a hypnogogic state.

We may conclude that while this experiment agrees with the work of Miller—its closest analogy—performance is shown to be affected by hyper-tension as well as by relaxation. The question of whether the two effects are of the same order must be referred to a later discussion.

Experiment VI

Problem. That muscular contraction, both general and local, may effect ensuing response is now well established. There is an extensive literature on the facilitation and inhibition of reflexes and of voluntary reactions by such means.[31] In the case of voluntary movement, the effect is frequently referred to as the 'law' of dynamogenesis. Very little is known, however,

[29] E. Jacobson, *ibid.*, 77, footnote 14.

[30] M. M. White, Relation of bodily tension to electrical resistance, *J. Exper. Psychol.*, 13, 1930, 267-277.

[31] Cf. the summaries in Jenkins, *op. cit.*, 3; and H. Peak, Modification of the lid-reflex by voluntarily induced sets, *Psychol. Monog.*, 42, 1931, (no. 188), 44-46.

of the relative facilitation received from the contraction of different muscle groups. While the work of Williams[32] and Judd[33] indicates that reaction-time is decreased by preliminary contraction of the reacting muscles, this effect has not been compared with contraction in other muscle groups. The present experiment sought to investigate the following question: Will tension in the reacting muscle facilitate reaction more or less than will tension in a remote group? The performance tested was that of rapid oscillation of the right index finger. The reader will recall that Experiment IV showed that the rate of oscillation was increased slightly when the left arm muscles were under tension. Here the tension of the quadriceps and flexor groups were systematically varied.

Apparatus. In preliminary work, photographic records were taken of the thickening of the quadriceps and the flexors of the fingers. The apparatus used was the same as is described in a previous communication,[34] with the *S* fastened in a modified 'pillory,' and with recording levers resting upon the four muscle groups under observation. In the major experiment records of finger oscillation were obtained by attaching a mechanical counter to the above device.

Procedure. The purpose of the preliminary records of thickening was to observe the spread of excitation incident to placing, in turn, each of the various muscle groups under the same amount of tension. A loosely wound spring with a resistance of approximately 6 lb. would be attached so as to place the desired muscle group under tension. (It was found that the muscle tensions could be kept more constant by this means than by use of weights as in Experiment IV, because of the relative lack of inertia of the spring.)

Four *S*s were used in the major experiment. Each repeated in a different time order, twenty 30-sec. periods of finger oscillation (right index finger) under each of the following experimental variations: (1) flexors of right middle finger under tension, (2) flexors of left middle finger under tension, (3) right quadriceps under tension, and (4) left quadriceps under tension.

Results. The photographic records of muscle thickening showed that the spread of excitation from the focal contraction was largely bilateral; that is, contraction of the right quadriceps resulted in a thickening of the left quadriceps, and the spread from the contraction of the left finger flexors was greatest to the flexors on the right side. This is presumptively due to associated reflex connections between the bilateral groups and is supported by certain observations on the normal knee-jerk,[35] and on the neuromuscular spread of cortically initiated activity.[36] Such bilateral spread

[32] F. Williams, *op. cit.*
[33] C. H. Judd, *op. cit.*
[34] *Freeman, op. cit., J. Gen. Psychol.*, 5, 1931, 479-494.
[35] Cf. the summary of this work by G. R. Wendt, An analytic study of the conditioned knee jerk, *Arch. Psychol.*, 18, 1930, 1-97.
[36] Freeman, *loc. cit.*

aids in explaining the relative amount of reënforcement derived from the contraction of the four muscle groups that we studied. Table XVI shows that the average number of finger oscillations per sec. is most rapid when the experimental tension is localized in a muscle near the reacting group. Oscillation is least rapid when experimental tension is localized in the anatomically and neurologically more remote quadriceps groups.

TABLE XVI

AVERAGE NUMBER OF FINGER OSCILLATIONS PER SECOND WITH EXPERIMENTALLY INDUCED TENSION

Time order	S	Tension localized in			
		R.m. finger	L.m. finger	Right Quad.	Left Quad.
1	*Ga*	6.1	5.6	6.5	6.1
2	*Ka*	5.9	5.8	4.2	5.1
3	*Al*	5.8	5.7	5.1	5.8
4	*Ke*	3.8	4.5	3.3	4.1
	Av.	5.4	5.4	4.8	5.3

Discussion. The fact that subject *Ke* did so much better with the left finger under tension than with the right calls for comment. Since the difference cannot be explained as due to time order, we suggest the increased tension in the reacting members may have worked an inhibition. Had we taken photographic records of the thickening of the various muscle groups, we should be able to supply more exact information upon this point. As it is, we can only give it as our opinion that the subjects followed instructions not to voluntarily increase the tension of other than the group which resisted the pull of the spring.

Our work must be regarded as more exploratory than definitive and our conclusions are necessarily limited to a specific situation. In the case of finger oscillation, our results indicate that the facilitating effect of tension varies inversely with the neurological remoteness of muscle group under tension from the group which is primarily involved in the performance. This would hold, of course, only for equivalent or nearly equivalent amounts of tension in the muscles under survey. The results suggest that the facilitative effect of many 'motor' sets may be due to such optimal localization of tension. An important field of research is thus open to a new avenue of approach.[37]

[37] We must be very critical of this type of research. Facilitation of any activity will depend upon three factors: (1) qualitative, the locus of the facilitating activity; (2) quantitative, the amount or intensity of the facilitating activity; and (3) temporal, the time relations existing between the onset of facilitating activity and the performance under observation. To obtain the qualitative locus of optimal muscular facilitation, the quantitative and temporal factors must be held constant. While this was partially accomplished in our research, more adequate controls need to be de-

Physiological Basis of Tonic Facilitation and Inhibition

We have thus far written of favorable influences as instances of facilitation and of unfavorable influences as inhibition. The physiological interpretation of these results, a matter of general interest, is our next concern. We summarize the evidence under four general headings.

(*1*) *Performance accompanied by an increase in recorded muscle tone* (Lombard, Muskens, Tuttle, Freeman, and others). The chief difficulty with this type of evidence is its lack of conclusiveness. For instance, there is just as good reason to suppose a priori that the recorded changes are incidental consequences rather than causal factors in performance. Indeed the first assumption seems the more probable. We know that neural excitation, when intense, may spread to centers not primarily involved in a task, thereby causing expressive movements and the like. But if this were the case, we should expect a slight latency in recorded tonus changes as well as a rising curve during performance; whereas, study of the time relations indicates that maximal tension usually coincides with task initiation or else is slightly anticipatory (Golla and Antonovitch) (Freeman) and the rising curve is not a predominating feature of recorded changes. These facts make it seem that the tonus accompaniments of work are a part of the 'set' for activity rather than the overflow of excitation from centers already in action. This question cannot be settled on the basis of the evidence offered.

(*2*) *Comparison of recorded tonus changes with performance during variations in mental effort* (Golla and Antonovitch, Bills, and Experiments I and II above). Effort varies with the *S*'s estimation of task difficulty and his motivation. Assuming the subject to be highly motivated, variations in the difficulty of a task produce significant correlations between recorded tension and performance scores. Golla and Antonovitch report negative results; but Bills showed that tension increased in the more difficult parts of maze learning by adults, and the present report showed that increased tension incident to preparation for the more difficult tasks was accompanied by increased efficiency in performance. In Experiment II above, tension was notably increased under conditions of high-pressure motivation with a detrimental effect upon performance. All these instances of facilitation and inhibition provide little information upon the mechanisms involved.

(*3*) *Comparison of individual differences in performance with recorded tonus changes* (Duffy, Johnson, Bills, Freeman, and Freeman and Lindley). The work of Bills and Freeman shows that adults with high tension scores perform as well if not better than individuals with low tension scores. This is somewhat contradictory to the results obtained by Duffy on children. The difference is more apparent than real. The chief difficulty experienced by children with high tension scores was in the coördination of movements. Since it is possible that adults may achieve coördination in spite of high tension, this excess might well become facilitative. The ques-

---

vised. Their necessity is recognized by other workers. Thus Peak (*op. cit.*, 53 f.), in observing that the amplitude of the lid-reflex to sound was greater under the 'lid-voluntary' than the 'finger-voluntary' reaction set, points out that her inference that the more proximate set has the greatest effect cannot be substantiated "by an experiment which does not control both the intensity of the facilitating activity and its temporal relation to the reflex in question."

tion of whether a given amount of tension will not inhibit performance in one individual and facilitate it in another cannot be answered by the evidence available. The necessity for further work is evident.

(*4*) *Comparison of performance under different states of experimentally induced tension* (Bills, Miller, Jacobson, and Experiments II, IV, V, and VI above). The most convincing evidence of muscular facilitation and inhibition comes from studies of this sort. Tension or relaxation is set up experimentally and performance under this condition is compared with a control series. Jacobson found that an *S* showed a diminution of conscious processes and reflex excitability under relaxation. Miller carried the problem further and reports that the effect of relaxation is to increase the reaction-time to electric shock, reduce the extent of movement and diminish the apparent intensity of the stimulus. Conversely, Bills demonstrated that the rate of learning various tasks was facilitated by increasing the tension experimentally. In Experiment V above, the effects of hyper-tension and of relaxation were investigated in the same *S*s. Irritability was lowered by relaxation and raised by hypertension. The accuracy of discrimination suffered under both of these extreme muscular conditions. Experiment IV indicated that the facilitative effect of tension depends somewhat upon the performance under survey. While finger oscillation was not effected by an excess of tension, a coördinated pursuit task was inhibited. Experiment III showed that the facilitative effect of preliminary contraction of the reacting muscle upon reaction-time. Experiment VI indicated that the facilitative effect of muscular tension (during finger oscillation) varies inversely with the remoteness of the contracting muscle from the reacting member. A careful consideration of the controls utilized in the above-mentioned experiments convinces one of the validity of muscular facilitation as the normal accompaniment of performance. Detrimental effects result from hyper- and hypo-tension.

We are now concerned with the neural energy transformations which produce the effects just described. The mechanism of tonic muscular facilitation is not difficult to conceive. If facilitation is defined as the increase of neural action by increased stimulation, the summation of impulses will explain the reënforcement of higher neural arcs by muscular tension. It is when we attempt to push our analysis further that the relation becomes less clear. Where and in what manner does this summation occur? It is a well-known fact that there is summation of impulses on the motor horn cells and in the cerebral cortex as well as on the afferent side.[38] Cortical summation of proprioceptive with impulses of other origin apparently occurred in many of our experiments. The facilitation of selected acts by the preliminary contraction of the reacting muscle groups indicates that summation may also have occurred on the efferent side of the arc. The condition of optimal facilitation found in Experiment VI was probably favored by the proximity of the excitation to the efferent part of the re-

[38] F. H. Pike and M. N. Chappell, *Science,* 71, 1930; M. N. Chappell, *Psychol. Rev.,* 38, 1931, 317-332. The original articles are given in these papers.

action pattern. That is, the effects of equivalent excitation in more remote neurological groups is largely dissipated before its 'spread' includes the reacting member. Where there is no associated reflex connection between the muscles under tension and those directly engaged in the performance, whatever facilitation does occur will likely result from summation at the cortical level.

The complexity of nervous organization permits the possibility of summation at any and all levels. Jacobson has discussed at length the rôle of various portions of the nervous system in the regulation of tone.[39] The long-circuiting of impulses favors the view that cortical centers play an important part in such regulation. The present writer has reviewed the evidence for assuming that the action of cortical centers is in turn regulated by afferent impulses of proprioceptive origin.[40] Reënforcement of reaction patterns by muscular tension is a common if not universal occurrence. The effect is, of course, more obvious in some performances than in others and depends partially upon their nature. Muscular acts would normally require more reënforcement than would action patterns limited almost exclusively to cerebral fields. Our records offer proof that such 'mental' reactions frequently involve an increase of excitation in a remote muscle group. We do not wish to imply, however, that every reaction must be accompanied by an increase in tone. The neural mechanism is more than a mere touch-and-go system and normal waking tone may provide sufficient reënforcement for many reactions without recourse to additional increment.

It may be useful to presume that tonic facilitation is quite automatic during normal waking adjustment, whereas those increments in tension of which we are often aware during periods of stress are of more voluntary origin. Of course it does not matter whether one says "I will contract my muscles because it will increase my efficiency," or whether one finds that his muscles are contracted without such voluntary direction. The crucial fact is that in both cases the reënforcement of performance may occur.

When we consider the problem of inhibition as related to our topic we are upon more difficult grounds. The inhibitory effects of tension are of two kinds (a) decreased efficiency with decreased tension, and (b) decreased efficiency with increased tension. Now decreased neural action by decreased stimulation is as axiomatic as its converse, increased neural action by increased stimulation. But decreased action by increased stimulation

[39] E. Jacobson, *Progressive Relaxation*, 1929, 220-269.
[40] G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449.

needs explanation. We can understand how the diminution of afferent impulses from the muscles may diminish production through lack of necessary reënforcement. This was apparently the case in Experiments II and V. The inhibitory effect of hyper-tension in part (b) of these two experiments is more difficult to understand.

Dodge[41] lists the following varieties of reaction decrement from increased stimulation: (1) terminal or peripheral inhibition; (2) reflex inhibition or the reciprocal effects of antagonistic muscle action; (3) refractory phase inhibition, where the second stimulus falls within the relative refractory period of reaction to the first; (4) inhibition by rivalry of impulses in competition for the final common path; (5) inhibition of weaker by stronger stimuli (Hyman's Law); (6) anode block, where a region of decrement is produced by persistent electrical stimulation; and (7) protracted central inhibition consequent to frequent stimulation. To this we might add another (8) inhibition by occlusion,[42] where the combined impulses from two sources exceeds that sufficient for full tetanic contraction of a muscle and so gives contraction decrement by overlapping excitatory effects.

With the problem of inhibition in its present confused state, it is difficult to say which, if any, of these varieties can be applied to diminished response with increased tension. Certain analogies can be drawn from each of the above descriptions, but the last variety seems most suggestive in the present context. Under this analogy, the neural pattern is conceived to permit a certain ratio in the summed impulses which sustain it. If the number of these impulses exceeds such limits, the pattern is 'fogged up' with a consequent decrement in performance. While the higher neural integrations demand a certain amount of sustaining action from the muscular processes, an excess of this action—by its very prominence in the neural flux—disturbs the equilibrium of the organization. The proprioceptive impulses do not gain entire control of the reacting mechanism, as would follow from the 'rivalry' analogy; rather they cut down or limit the efficacy of the total reaction pattern.

The details of this analogy are necessarily vague. Yet the principle can be profitably applied to a number of facts which we have observed. Consider, for example, the fact that a performance requiring a high degree of coördination is 'inhibited' by hyper-tension, while a less difficult task may be 'facilitated' under similar conditions (Experiment IV). May not the

[41] R. Dodge, The problem of inhibition, *Psychol. Rev.*, 33, 1926, 1-12.
[42] C. S. Sherrington, Quantitative management of contraction in lowest level coordination, *Brain*, 54, 1931, 1-28.

BOSTON UNIVERSITY
COLLEGE OF LIBERAL ARTS
LIBRARY

first pattern be of such nature that the visual and kinaesthetic impulses must preserve a definite balance in order for manual pursuit to be properly executed? The effect of increasing the general background of tension might be that selected kinaesthetic impulses (from the reacting member) become confused with other impressions of this same modality and so lose some of their directive control.

Much remains to be known of the neural mechanisms responsible for the effects observed in our own and others' experiments; and although this aspect is particularly dependent upon the results of physiological experimentation, the psychologist must assume his share of the responsibility. We can no longer be content with the non-technical use of the terms *facilitation* and *inhibition* as applied to the differential effects of tension upon performance.

FURTHER EXPERIMENTS IN PHONETIC SYMBOLISM

By STANLEY S. NEWMAN, University of Chicago

Many persons believe that phonetic elements, apart from their function in a particular linguistic context, carry with them certain symbolic connotations.[1] The vowels *o* and *u*, for instance, are said to be heavier, gloomier, more ponderous than *i* and *e*.

Although one might extend indefinitely this type of phonetic interpretation, one cannot escape the conviction that normal individuals would tend to disagree considerably in the specific symbolism associated with any vowel or consonant. However true this suspicion of individual difference may be, it does not follow that phonetic sounds in themselves have no consistent symbolic relation outside of their linguistic associations. The two problems are generally confused, and both are branded with the stigma of being unreliably intuitive. The point is stressed because I found, from speaking to a number of the students acting as subjects in this investigation, that their assurance on this matter was at striking variance with their reactions in the experiments. They seemed to feel that the process disclosing the symbolic implications of phonetic elements was in some way a highly variable and arbitrary intuitive association. And this point of view, I am sure, reflects common opinion.

But the practical use of this extra-linguistic function of phonetic sounds is familiar enough in literature. The contemporary minor poet, for example, uses great ingenuity in employing this device to gain his effects.

This paper is an attempt to study the patterning of phonetic symbolisms on a non-linguistic plane. For this purpose a schedule was devised employing paired nonsense words. The pairs were so arranged that they would be similar in every respect but for the two vowels or consonants that were to be contrasted, *e.g. glupa: glopa.* The arbitrary meaning "horse"

* Accepted for publication June 1, 1931. This study was sponsored by the Social Science Research Committee at the University of Chicago.

[1] Edward Sapir, A study in phonetic symbolism, *J. Exper. Psychol.*, 12, 1929, 225-239. I am deeply indebted to Professor Sapir for his assistance in this study. The data of Experiment I were collected by him and handed over to me for inclusion in this paper; a preliminary analysis of some of that material will be found on pages 229-236 of his article.

was applied to this pair, and the subjects were asked to choose whether the first word of the pair signified to them the larger or the smaller horse. The subjects were given a sheet with consecutive pairs of numbers; after the investigator pronounced the word pair and gave the arbitrary meaning, the subjects recorded their vote by checking the appropriate number.

TABLE I

KEY TO PHONETIC SYMBOLS

Experiment I

i	as in French	*fini*	
e	" "	"	*été*
ɛ	" "	English	*met*
ä	" "	"	*hat*
a	" "	German	*Mann*
u	" "	English	*put*
o	" "	French	*tôt*
ɔ	" "	"	*note*

Experiment II

ɪ	as in English		*fit*
i·	" "	"	*seize*
ai	" "	"	*right*
ei	" "	"	*rate*
yu·	" "	"	*huge*
ä·	" "	"	*sad*
u·	" "	"	*pool*
a·	" "	"	*father*
ɔ·	" "	"	*tall*

c	as *sh* in English		*short*
tc	" *ch* "	"	*churn*
θ	" *th* "	"	*thin*
ð	" *th* "	"	*then*
j	" *z* "	"	*azure*
dj	" *g* "	"	*germ*

All other consonantal symbols had their common English value.

The statistical method employed in treating the data is a psychophysical method arranged by L. L. Thurstone.[2] The procedure consists essentially of reducing the experimental proportions to sigma scores on a normal curve of distribution. The mean sigma separations between adjacent stimuli are computed, and a final scale is then made from the accumulated steps of the individual stimuli.

Experiment I. One hundred word-pairs were used. Each phonetic contrast, however, was presented more than once throughout the hundred pairs; so that the proportions (See Table II) are average percentages of 2 to 5 responses for the given contrast.

The symbolism, on the basis of which the *S*s were to make their choice, was that of large versus small.

A variable was introduced in the form of concept classes; the arbitrary meanings of the first 40 pairs were nouns (large versus small objects), the next 10 pairs were verbal subjects (adult versus child activity), the next 10 were adjectives (qualities as applied to large versus small objects), etc. But a comparison of the proportions on the basis of these conceptual categories showed that the variable had no apparent effect on the responses.

The major purpose of this experiment, then, was to discover whether

[2] For a more complete exposition of method see L. L. Thurstone, An experimental study of nationality preferences, *J. Gen. Psychol.*, 1, 1928, 405-425. The reader will find a detailed analysis of the logic underlying this procedure in other articles by the same author. Professor Thurstone has generously given personal advice on many points of statistical method involved in this paper.

individuals tended to pattern phonetic sounds on the basis of size symbolism. Another purpose was to find whether age differences affected the patterning. The 606 *Ss*[3] taking part in the experiment were divided into three age groups:

Group 1 9-13 yr. old (223 *Ss*)
Group 2 14-15 yr. old (218 *Ss*)
Group 3 16 yr. old and above (153 *Ss*)

The consonant pairs were not of a sufficient number to permit the calculation of a scale, but will be used in Experiment II.

The tabulated results may be interpreted as follows. In Group 1, 32.2% of the *Ss* voted that *i* was larger than *e* (Proportion $i > e = 0.322$, Table

TABLE II

EXPERIMENTAL PROPORTIONS

		i	e	ɛ	ä	a	u	o	ɔ
	i		.678	.757	.812	.802	.772		
	e	.322		.564		.742		.732	
	ɛ	.243	.436		.713	.808			.782
	ä	.188		.287		.576	.637	.660	.717
Group 1	a	.198	.258	.192	.424		.446	.600	.656
	u	.228			.363	.554		.666	.595
	o		.268		.340	.400	.334		.497
	ɔ			.218	.283	.344	.405	.503	
	i		.705	.774	.801	.828	.871		
	e	.295		.592		.705		.761	
	ɛ	.226	.408		.718	.746			.847
	ä	.199		.282		.579	.591	.661	.659
Group 2	a	.172	.295	.254	.421		.486	.598	.642
	u	.129			.409	.514		.656	.595
	o		.239		.339	.402	.344		.450
	ɔ			.153	.341	.358	.405	.550	
	i		.708	.796	.864	.804	.870		
	e	.292		.593		.692		.748	
	ɛ	.204	.407		.742	.747			.806
	ä	.136		.258		.577	.559	.637	.706
Group 3	a	.196	.308	.253	.423		.501	.606	.567
	u	.130			.441	.499		.598	.590
	o		.252		.363	.394	.402		.516
	ɔ			.194	.294	.433	.410	.484	
	i		.696	.773	.821	.812	.834		
	e	.304		.582		.715		.747	
	ɛ	.227	.418		.722	.769			.812
	ä	.179		.278		.577	.600	.654	.693
Average	a	.188	.285	.231	.423		.475	.601	.628
	u	.166			.400	.525		.645	.594
	o		.253		.346	.399	.355		.485
	ɔ			.188	.307	.372	.406	.515	

[3] Not included in this grouping are twelve schedules received from Chinese students; these will be held until further work is done with phonetic symbolisms among foreign-speaking individuals.

TABLE III

Sigma Scores

		i	*e*	*ɛ*	*æ*	*ɛ*	*ä*	*ä*	*u*	*ʉ*	*a*	*ɑ*	*o*	*ɒ*	*ɔ*
Group 1	*i*	.00	.46	.46	.70	.70	.89	.89	.75	.75	.85				
	e	−.46	.00	.00	.16							.65	.62		
	ɛ	−.70	−.16	−.16	.00	.00	.56								
	ä					−.56	.00	.00	.35	.35	.19	.19	.41	.41	.57
	a	−.85	−.65	−.65	−.87	−.87	−.19	−.19	−.14	−.14	.00	.00	.25	.25	.40
	ʉ							−.35	.00	.00	.14	.14	.43	.43	.24
	o							−.41	−.43	−.43	−.25	−.25	.00	.00	.01
	ɔ					−.78	−.57	−.57	−.24	−.24	−.40	−.40	−.01	−.01	.00
		−2.01	−.35	−.35	−.01	−1.51	.69	−.63	.29	.29	.53	.33	1.70	1.08	1.22

Scale Separations	
i-e	.5869
e-ɛ	.1202
ɛ-ä	.6222
ä-ʉ	.2168
ʉ-a	.0566
a-o	.3229
o-ɔ	.0396

		i	*e*	*ɛ*	*æ*	*ɛ*	*ä*	*ä*	*a*	*ɑ*	*u*	*ʉ*	*ɔ*	*ɒ*	*o*
Group 2	*i*	.00	.54	.54	.75	.75	.85	.85	.95	.95	1.13				
	e	−.54	.00	.00	.23										
	ɛ	−.75	−.23	−.23	.00	.00	.58	.58	.66						
	ä					−.58	.00	.00	.20	.20	.23	.23	.41	.41	.42
	a	−.95	−.54	−.54	−.66	−.66	−.20	−.20	.00	.00	−.04	−.04	.36	.36	.25
	ʉ							−.23	.04	.04	.00	.00	.24	.24	.40
	o							−.42	−.25	−.25	−.40	−.40	−.13	−.13	.00
	ɔ					−1.02	−.41	−.41	−.36	−.36	−.24	−.24	.00	.00	.13
		−2.24	−.23	−.23	.32	−1.51	.82	.17	1.24	.58	.68	−.45	.88	.88	1.20

Scale Separations	
i-e	.7106
e-ɛ	.1944
ɛ-ä	.6590
ä-a	.2162
a-ʉ	.0236
ʉ-ɔ	.3762
ɔ-o	.0905

TABLE III (continued)

Group 3

	i	e	e	ɛ	ɛ	ä	ä	a	a	u	u	o	o	ɔ
i	.00	.55	.55	.83	.83	1.10	1.10	.86	.86	1.13				
e	−.55	.00	.00	.24										
ɛ	−.83	−.24	−.24	.00	.00	.65	.65	.66						
ä					−.65	.00	.00	.19	.19	.15	.15	.35	.35	.54
a	−.86	−.50	−.50	−.66	−.66	−.19	−.19	.00	.00	.00	.00	.27	.27	.17
u							−.15	.00	.00	.00	.00	.25	.25	.23
o							−.35	−.27	−.27	−.25	−.25	.00	.00	.04
ɔ					−.86	−.54	−.54	−.17	−.17	−.23	−.23	−.04	−.04	.00
	−2.24	−.19	−.19	.41	−1.34	1.02	.52	1.27	.61	.80	−.33	.83	.83	.98

Scale Separations	
i-e	.7248
e-ɛ	.2121
e-ä	.6675
ä-a	.1515
a-u	.0448
u-o	.3281
o-ɔ	.0424

Average

	i	e	e	ɛ	ɛ	ä	ä	a	a	u	u	ɔ	ɔ	o
i	.00	.51	.51	.75	.75	.92	.92	.89	.89	.97				
e	−.51	.00	.00	.21										
ɛ	−.75	−.21	−.21	.00	.00	.59	.59	.74						
ä					−.59	.00	.00	.19	.19	.25	.25	.50	.50	.40
a	−.89	−.57	−.57	−.74	−.74	−.19	−.19	.00	.00	−.06	−.06	.33	.33	.26
u							−.25	.06	.06	.00	.00	.24	.24	.37
o							−.40	−.26	−.26	−.37	−.37	−.04	−.04	.00
ɔ					−.89	−.50	−.50	−.33	−.33	−.24	−.24	.00	.00	.04
	−2.15	−.27	−.27	.22	−1.47	.82	.17	1.29	.55	.55	−.42	1.03	1.03	1.07

Scale Separations	
i-e	.6647
e-ɛ	.1728
e-ä	.6477
ä-a	.2274
a-u	.0000
u-ɔ	.3841
ɔ-o	.0113

II, top of *i*-column). This proportion corresponds to the sigma score —0.46 (Table III). The scale separation between *i* and *e* is the mean difference between the repeated sigma scores of the two stimuli multiplied by $2^{1/2}$, the discriminal errors of the stimuli.[4]

$$\begin{aligned} S_1 - S_2 &= x_{12}(2)^{1/2} \\ &= [(-.35 \times 2.01)/4](2)^{1/2} \\ &= (1.66/4)(2)^{1/2} \\ &= .415 \times (2)^{1/2} \\ &= .5869 \end{aligned}$$

The scale values are derived from the accumulated scale separations between adjacent stimuli (Table IV). The position of any vowel on the

TABLE IV

SCALE VALUES

	Group 1	Group 2	Group 3	Av.	Median*
i	.0000	.0000	.0000	.0000	.0000
e	.5869	.7106	.7248	.6647	.7106
ɛ	.7071	.9050	.9369	.8375	.9050
ä	1.3293	1.5640	1.6044	1.4852	1.5640
a	1.6027	1.7802	1.7559	1.7126	1.7802
u	1.5461	1.8038	1.8007	1.7126	1.8038
o	1.9256	2.2705	2.1288	2.1080	2.1833
ɔ	1.9652	2.1800	2.1712	2.0967	2.2229

* The Median Scale Values are determined on the basis of median scale separations.

symbolic scale is a medial point computed from its various positions when contrasted with individual vowels.

It is further possible to determine the range of shifting that each item undergoes: thus, *i* will take one position on the scale when contrasted with *e*, another when contrasted with ɛ, etc. The scale separation 0.5869, or 0.415 σ, between *i* and *e* corresponds to the proportion 0.339 for *i*. The difference between the experimental proportion and the calculated proportion (Pe—Pc) is —0.017 (.322—.339 = —.017). Such a method of computing discrepancies is, as one may assume, a test of the internal consistency of the responses.

The subjective scales of the three groups reveal the same essential patterning (Fig. 1). The vowel *i* always occurs relatively far below the succeeding vowel *e*, which is followed rather closely by ɛ, after which comes *ä* separated by another considerable distance. Slight inconsistencies appear in the allocation of *a* against *u* and again in *o* against ɔ; but these two groups of vowels are close together on each of the three scales, and the inconsistencies, falling well within the average discrepancy area, are further reflected in a comparison of discrepancy ranks.

[4] Thurstone, *op. cit.*, p. 407.

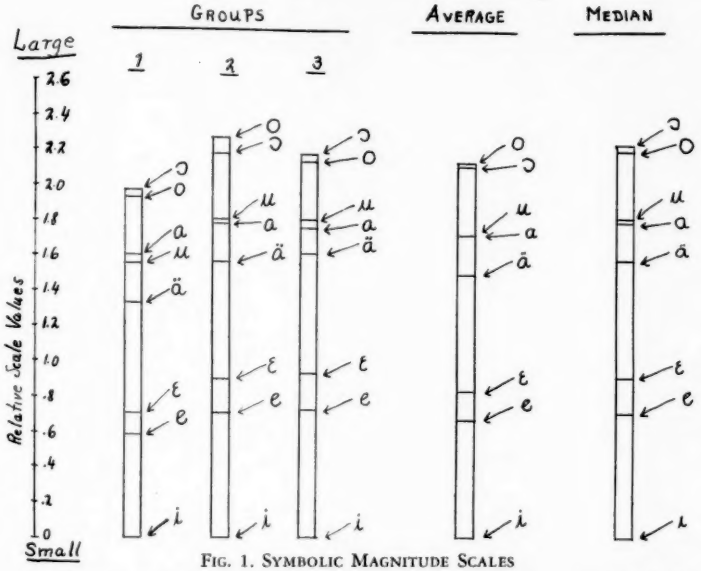


FIG. 1. SYMBOLIC MAGNITUDE SCALES

The discrepancies in many ways tell a significant story. An examination of the average discrepancies (Table V) indicates that the youngest group shows the greatest degree of inconsistency (0.041), the older group displays less (0.035), and the oldest group has the least (0.032). This is a situation one might expect; and it bears out the fact that the scale separations of Group 1 are the smallest, this group tending to discriminate between the vowels less distinctly than the other groups. But it should be observed that these scale and discrepancy differences between the age-groups are only slight differences in the clarity and stability of discriminal perception, not in fundamental pattern. In other words, the youngest group has substantially the same subjective pattern as the older groups, but it tends to discriminate less sharply and less consistently than they do between the sounds.

A comparison of the individual discrepancies, however, suggests that the use of these to explain the range of instability interprets only a part of the evidence. In the majority of cases the shifting appears to be of the same general trend. A few examples are listed:

	Groups			Av.
	1	2	3	
i>e	−.017	−.013	−.012	−.015
i>ɛ	−.066	−.035	−.050	−.050
i>a	.069	.068	.089	.075
e>ɛ	−.030	−.037	−.033	−.033
e>o	.096	.104	.092	.099
ɛ>ä	−.043	−.039	−.060	−.045
ä>o	.003	.030	.008	.016
a>u	.038	.021	.012	.025
u>ɔ	.021	.010	.013	.013

These phenomena indicate that the discrepancies point not only to chance fluctuations and inconsistencies but to constant shifts. The reasons behind such constant variability cannot be satisfactorily explained from the details of the available data; but it is significant to note that even the low discrepancy of the average scale

TABLE V

DISCREPANCIES

(Pe—Pc)

		i	*e*	*ɛ*	*ä*	*a*	*u*	*o*	*ɔ*	*Total*
Group 1	*i*		.017	.066	−.014	−.069	−.091			
	e	−.017		.030		−.022		−.096		
	ɛ	−.066	−.030		.043	.071			−.031	
	ä	.014		−.043		−.001	.076	−.003	.043	
	a	.069	.022	−.071	.001		−.038	.010	.055	
	u	.091			−.076	.038		.060	−.021	
	o		.096		.003	−.010	−.060		−.014	
	ɔ			.031	−.043	−.055	.021	.014		
Σ(Pe−Pc)		.257	.165	.241	.180	.266	.286	.183	.164	1.742
Av.		.051	.041	.048	.030	.038	.057	.037	.033	.041
Group 2	*i*		.013	.035	−.065	−.068	−.028			
	e	−.013		.037		−.070		−.104		
	ɛ	−.035	−.037		.039	.014			.031	
	ä	.065		−.039		.018	.024	−.030	−.009	
	a	.068	.070	−.014	−.018		−.021	−.038	.031	
	u	.028			−.024	.021		.027	−.010	
	o		.104		.030	.038	−.027		−.025	
	ɔ			−.031	.009	−.031	.010	.025		
Σ(Pe−Pc)		.209	.224	.156	.185	.260	.110	.224	.106	1.474
Av.		.042	.056	.031	.031	.037	.022	.045	.021	.035
Group 3	*i*		.012	.050	−.008	−.089	−.029			
	e	−.012		.033		−.075		−.092		
	ɛ	−.050	−.033		.060	.028			−.003	
	ä	.008		−.060		.034	.004	−.008	.050	
	a	.089	.075	−.028	−.034		−.012	.002	−.048	
	u	.029			−.004	.012		.006	−.013	
	o		.092		.008	−.002	−.006		.004	
	ɔ			.003	−.050	.048	.013	−.004		
Σ(Pe−Pc)		.188	.212	.174	.164	.288	.064	.112	.118	1.330
Av.		.038	.053	.035	.027	.041	.013	.022	.024	.032
Average	*i*		.015	.050	−.032	−.075	−.053			
	e	−.015		.033		−.056		−.099		
	ɛ	−.050	−.033		.045	.037			−.002	
	ä	.032		−.045		.013	.036	−.016	.026	
	a	.075	.056	−.037	−.013		−.025	−.009	.021	
	u	.053			−.036	.025		.035	−.013	
	o		.099		.016	.009	−.035		−.012	
	ɔ			.002	−.026	−.021	.013	.012		
Σ(Pe−Pc)		.225	.203	.167	.168	.236	.162	.171	.074	1.406
Av.		.045	.051	.033	.028	.034	.032	.034	.015	.033

(0.033) is probably much too high if one takes this figure merely as an index of inconsistency. We are evidently dealing with a field of latent symbolism in which the patterning is even more rigid and constant than the discrepancies would lead us to believe.

A consideration of the discrepancies from another point of view helps to allocate the scale-inconsistencies referred to above. The vowel *u* falls below *a* on the scale of Group 1, contradicting the alignment of these vowels on the scales of the other two groups; likewise, the vowel *o*, in appearing above ɔ on the scale of Group 2, falls out of the pattern set by the other two groups. If the 8 vowels are arranged in rank order of their discrepancy-scores within each group, the *u* of Group

TABLE VI

DISCREPANCY-RANK

	Groups				
	1	2	3	Av.	Median
i	7	6	6	7	6
e	5	8	8	8	8
ɛ	6	4	5	4	5
ä	1	3	4	2	3
a	4	5	7	5	5
u	8*	2	1	3	2
o	3	7*	2	6	3
ɔ	2	1	3	1	2

1 and the *o* of Group 2 will be found to have a much higher discrepancy rank than these vowels in the other groups. The two starred examples, it will be noted, depart farther from a median discrepancy rating than any of the other examples in the entire set. We may assume, then, that the normal position of *u* is slightly above *a* on the subjective scale, and *o* is slightly below ɔ; since in each case we are taking as the norm the choice of two groups having a low discrepancy-rank for these vowels as against that of one group having a strikingly high discrepancy-rank.

In general, the rationale which underlies the symbolic magnitude-pattern appears to be of a mechanical nature. The front vowels of the symbolic scale follow the order, *i, e, ɛ, ä, a*. This sequence is similar to a vowel-series following (1) the receding positions of articulation made by the tongue within the mouth, (2) the decreasing frequencies of vocalic resonance as measured acoustically, and (3) the increasing size of the oral cavity used in pronunciation. Thus far the three factors work in unison to present the same picture of a vowel sequence as that found on the symbolic scale. But these factors are at variance in their arrangement of the back vowels. The situation might be schematized as follows:

(1) Kinesthetic factor: articulatory position of tongue (front to back), *i, e, ɛ, ä, a, ɔ, o, u*.

(2) Acoustic factor: characteristic high frequency of vocalic resonance (high to low),[5] *i, e,* ɛ, *ä, a,* ɔ, *o, u.*

(3) Kinesthetic or, possibly, visual factor: size of oral cavity (small to large), *i, e,* ɛ, *ä, a; u, o,* ɔ.

The symbolic magnitude-pattern conforms to the first two series in placing the back vowels above the front vowels; but it compensates for this arrangement, as it were, by following the sequence of the third series. But the group-scales are not entirely consistent in their sequence of back vowels, as was pointed out above; furthermore, their arrangement of back vowels leans more heavily on the patterns of factors 1 and 2—the matter of placing the back vowels above the front being a more thoroughgoing spatial discrimination than the matter of sequence. For these reasons we may assume that the size of the oral cavity is a less potent factor in judgments of magnitude-symbolism than the accompanying factors of articulatory position and acoustic resonance.

Experiment II. A related experiment was conducted to determine the bearing of unlike symbolisms on the subjective patterns of phonetic sounds. The symbolisms chosen were (A) large versus small, and (B) dark versus bright.

The experimental and statistical procedure was essentially the same as that of Experiment I. A schedule of 113 word-pairs was drawn up, contrasting 9 vowels and 15 consonants. Each contrasting pair of vowels and of consonants was presented only once, in distinction to the previous experiment. Another difference of procedure was that the phonetic elements in this experiment were restricted to English sounds. The purpose of this restriction was to permit, later on, a comparison of the distribution of sounds on a symbolic scale with their distribution in meaningful English words. The *S*s were asked to choose the symbolically 'larger' word of each pair during the first half of the experiment; the same pairs were then given in random order and the *S*s were asked to choose the 'darker' word.

TABLE VII

u ' Contrasts

Contrasts	*Position*	*Accent*	*Syl. Structure*	
sú 'ha:síha	1st syl.	acc.	2 syl. word	
lä 'bɪ:lu 'bɪ	" "	unacc.	" " "	
hú 'len:há 'len	" "	acc.	" " "	final cons.
péiksa:pú 'ksa	" "	"	" " "	cons. doublet
ka'ú ':kaláɪ	2nd syl.	"	" " "	
dágu' :dágɔ '	" "	unacc.	" " "	
dzu 'c:dzyu 'c			1 syl. word	

[5] Richard Paget, *Human Speech,* 1930, 86-93, 315-317. No adequate acoustic analysis has been made of the entire vocalic gamut for one voice. This acoustic scale has been assembled from the frequency-data of several voices; it is only an approximation, valid enough as an index of relative position, although it cannot claim any absolute numerical validity.

Because the total experiment contained 226 word-pairs, it was decided to vary the phonetic structure of the words in order to prevent, as far as possible, a deadening of the *Ss*' attention. The contrasted sounds were varied as to position in the word, accent, and syllabic structure. In the vowel-pairs, for instance, the contrasted elements might take any of the forms given in Table VII. But these variations were introduced consistently into the sounds, so that, if they should prove to be conditions affecting the responses, their total effect on the symbolic scale would be constant and uniform, although their individual values could still be computed.

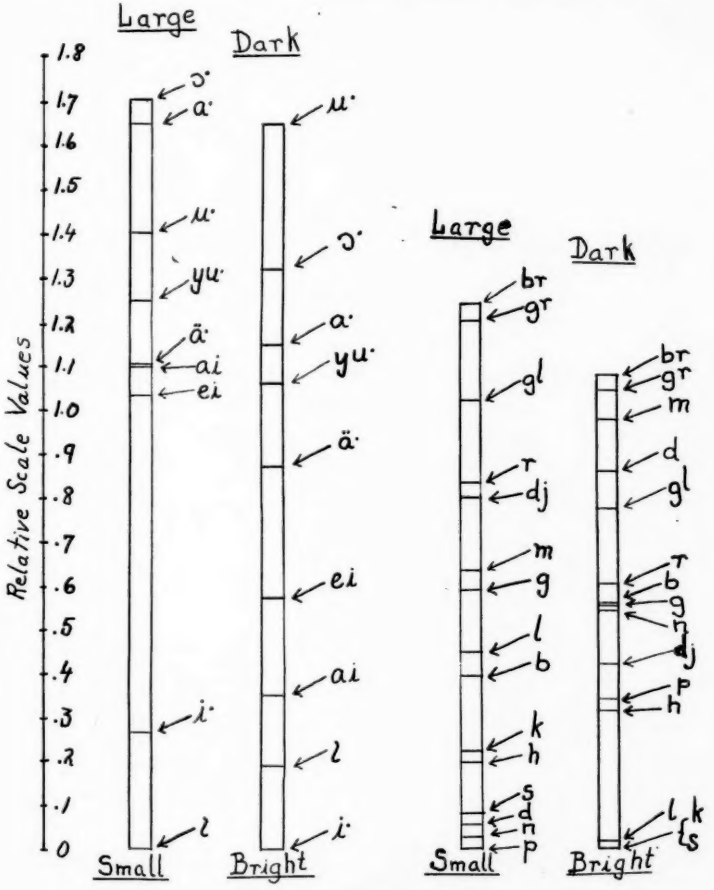


FIG. 2. SYMBOLIC SCALES

TABLE VIII

EXPERIMENTAL PROPORTIONS

Experiment 2A (Large vs. Small)

	ɛ	i·	ei	ai	ä·	yu·	u·	a·	ɔ·
ɛ		.456	.732	.775	.872	.721	.832	.920	
i·	.544		.779	.638	.737			.796	.755
ei	.268	.221			.623		.493	.759	.572
ai	.225	.362			.406	.500	.635		.667
ä·	.128	.263	.377	.594		.629	.607		
yu·	.279			.500	.371		.521	.610	.624
u·	.168		.507	.365	.393	.479		.532	.589
a·	.080	.204	.241			.390	.468		.655
ɔ·		.245	.428	.333		.376	.411	.345	

	p	n	d	s	h	k	b	l	g	m	dj	r	gl	gr	br
p		.511	.279		.593	.518	.773	.594	.851	.780	.580	.667		.809	.794
n	.489		.631	.566		.567	.775	.676	.467	.630	.709	.725	.695	.739	
d	.721	.369		.358					.655	.515	.688	.796	.674	.808	.757
s		.434	.642		.552	.387	.529	.629	.766	.713		.613		.787	.746
h	.461			.448		.544	.471	.736	.586	.714	.553		.746	.645	.766
k	.482	.433		.613	.465		.686	.277	.727		.560		.717	.674	.759
b	.227	.225		.471	.529	.314			.514		.752	.587	.723	.745	.872
l	.406	.471		.371	.264	.723			.442		.555	.650	.681	.745	.627
g	.149	.533	.345	.234	.414	.273	.486	.558		.438		.645	.664	.783	
m	.220	.370	.485	.287	.286				.562		.695	.589	.553	.581	.734
dj	.420	.291	.312		.447	.440	.248	.445		.305		.464	.606		.660
r	.333	.275	.204	.387			.413	.350	.355	.411	.536		.471	.783	
gl		.305	.326		.254	.283	.277	.319	.336	.447	.394	.529			.445
gr	.191	.261	.192	.213	.355	.326	.255	.255	.217	.419		.217			.539
br	.206		.243	.254	.234	.241	.128	.372		.266	.340		.555	.461	

TABLE IX

Experimental Proportions

Experiment 2B (Dark vs. Bright)

	i'	*ɪ*	*ai*	*ei*	*ä'*	*yu'*	*a'*	*ɔ'*	*u'*
ɪ'		.489	.573	.706	.793		.754	.776	
ɪ	.511		.515	.537	.626	.765	.772		.846
ai	.427	.485			.567	.659		.755	.836
ei	.294	.463			.625		.689	.706	.689
ä'	.207	.374	.433	.375		.517			.698
yu'		.235	.341		.483		.661	.487	.643
a'	.246	.228		.311		.339		.678	.687
ɔ'	.224		.245	.294		.513	.322		.621
u'		.154	.164	.311	.302	.357	.313	.379	

	k	*s*	*l*	*h*	*p*	*dj*	*n*	*g*	*b*	*r*	*gl*	*d*	*m*	*gr*	*br*
k		.518	.440	.575	.526	.593	.726	.810	.717		.621			.548	.644
s	.482		.543	.544			.625	.548	.748	.625		.831	.654	.705	.755
l	.560	.457		.405	.575	.748	.493	.515		.675	.586			.816	.776
h	.425	.456	.595		.452	.365		.474	.493		.674		.652	.741	.664
p	.474		.425	.548		.544	.507	.513	.522	.500		.500	.754	.694	.646
dj	.407		.252	.635	.456		.595		.395	.640	.652	.684	.716		.379
n	.274	.375	.467		.493	.405		.415	.582	.566	.419	.504	.597	.689	
g	.190	.452	.485	.526	.487		.585		.412	.461	.496	.483	.434	.735	
b	.283	.252		.507	.478	.605	.418	.588		.419	.569			.629	.741
r		.375	.325		.500	.360	.434	.539	.581		.647	.328	.461	.770	
gl	.379		.414	.326		.348	.581	.504	.431	.353		.537	.560		.610
d		.169			.500	.316	.496	.517		.672	.463		.581	.368	.593
m		.346		.348	.246	.284	.403	.566		.539	.440	.419		.544	.478
gr	.452	.295	.184	.259	.306		.311	.265	.371	.230		.632	.456		.614
br	.356	.245	.224	.336	.354	.621			.259		.390	.407	.522	.386	

TABLE X

An Example of Scale Calculation

Vowels of Experiment 2A (Large vs. Small)

Sigma Scores

Original Calculation

	ɪ	i˙	i˙	ei	ei	ä˙	ä˙	ai
ɪ	.00	−.11	−.11	.62	.62	1.14	1.14	.76
i˙	.11	.00	.00	.77	.77	.63	.63	.35
ei	−.62	−.77	−.77	.00	.00	.31		
ä˙	−1.14	−.63	−.63	−.31	−.31	.00	.00	.24
ai	−.76	−.35					−.24	.00
yu˙							−.33	.00
u˙					.02	−.27	−.27	−.35
a˙	−1.41	−.83	− 83	−.70				
ɔ˙			−.69	−.18				
	−3.82	−2.69	−3.03	.20	1.10	1.81	.93	1.00

	ai	yu˙	yu˙	u˙	u˙	a˙	a˙	ɔ˙
ɪ	.76	.59	.59	.96	.96	1.41		
i˙							.83	.69
ei					−.02	.70	.70	.18
ä˙	.24	.33	.33	.27				
ai	.00	.00	.00	.35				
yu˙	.00	.00	.00	.05	.05	.28	.28	.32
u˙	−.35	−.05	−.05	.00	.00	.08	.08	.22
a˙			−.28	−.08	−.08	.00	.00	.40
ɔ˙	−.43	−.32	−.32	−.22	−.22	−.40	−.40	.00
	.22	.55	.27	1.33	.69	2.07	1.49	1.81

Supplemented Calculation

	ɪ	i˙	ei	ai	ä˙	yu˙	u˙	a˙	ɔ˙
ɪ	.00	−.11	.62	.76	1.14	.59	.96	1.41	1.37*
i˙	.11	.00	.77	.35	.63	.75*	.90*	.83	.69
ei	−.62	−.77	.00	.15*	.31	.21*	−.02	.70	.18
ai	−.76	−.35	−.15*	.00	−.24	.00	.35	.44*	.43
ä˙	−1.14	−.63	−.31	.24	.00	.33	.27	.45*	.50*
yu˙	−.59	−.75*	−.21*	.00	−.33	.00	.05	.28	.32
u˙	−.96	−.90*	.02	−.35	−.27	−.05	.00	.08	.22
a˙	−1.41	−.83	−.70	−.44*	−.45*	−.28	−.08	.00	.40
ɔ˙	−1.37*	−.69	−.18	−.43	−.50*	−.32	−.22	−.40	.00
	−6.74	−5.03	−.14	.28	.29	1.23	2.21	3.79	4.11

* Supplemented sigma scores, calculated from the original scale

TABLE XI

Scale Values

Experiment 2A (Large vs. Small)

ɪ	.0000	p	.0000	m	.6354
i˙	.2687	n	.0254	dj	.8023
ei	1.0370	d	.0555	r	.8362
ai	1.1029	s	.0800	gl	1.0210
ä˙	1.1045	h	.1960	gr	1.2058
yu˙	1.2521	k	.2243	br	1.2464
u˙	1.4061	b	.3950		
a˙	1.6544	l	.4497		
ɔ˙	1.7047	g	.5901		

TABLE XII

SCALE VALUES

Experiment 2B (Dark vs. Bright)

i˙	.0000	k	.0000	r	.6016
ɪ	.1918	s	.0000	gl	.7780
ai	.3520	l	.0151	d	.8590
ei	.5736	h	.3159	m	.9778
ä˙	.8752	p	.3395	gr	1.0438
yu˙	1.0606	dj	.4197	br	1.0816
a˙	1.1534	n	.5413		
ɔ˙	1.3247	g	.5545		
u˙	1.6531	b	.5573		

TABLE XIII

AVERAGE DISCREPANCIES

Experiment 2A (Large vs. Small)

ɪ	.029	p	.079	m	.074
i˙	.059	n	.055	dj	.073
ei	.061	d	.086	r	.062
ai	.032	s	.064	gl	.048
ä˙	.041	h	.056	gr	.067
yu˙	.037	k	.078	br	.049
u˙	.046	b	.079		
a˙	.040	l	.063		
ɔ˙	.049	g	.075		
Av.	.044			Av.	.067

TABLE XIV

AVERAGE DISCREPANCIES

Experiment 2B (Dark vs. Bright)

i˙	.021	k	.080	r	.089
ɪ	.032	s	.062	gl	.051
ai	.024	l	.062	d	.086
ei	.039	h	.046	m	.065
ä˙	.020	p	.043	gr	.083
yu˙	.065	dj	.108	br	.078
a˙	.053	n	.067		
ɔ˙	.056	g	.072		
u˙	.034	b	.062		
Av.	.038			Av.	.070

Experiment 2A (large versus small) was taken by 141 *Ss*, undergraduates at the University of Chicago. Five dropped out after completing Experiment 2A, leaving 136 in Experiment 2B (dark versus bright).

A casual examination of the experimental proportions was sufficient to show that the variations referred to above had a decided effect on the responses (Tables VIII and IX). It was necessary then to supplement the original sigma calculation with one which would make use of all the data, in order that each stimulus-position on the scale would be the result of the total conditions. An example of the two types of calculation is given

in Table X. The final supplemented scale-values are to be found in Tables XI and XII and Fig. 2.

A comparison of the vowel-constellations of the two symbolic scales clearly shows two different patterns. The magnitude scale of the previous experiment appeared to be based upon three objective factors: articulatory position, acoustic frequency, and size of oral cavity. A point for point comparison of the symbolic scales of Experiment 1 and Experiment 2A is not possible, of course, for the vowels are different. But it will be noted that the same general plan is confirmed; the small-to-large symbolism follows the sequence of receding tongue position, decreasing characteristic high frequency, and increasing size of the oral cavity. The present experiment, however, reveals another factor entering into the judgment of size symbolism; *i.e.* vocalic length. The position of *i·* above ɪ on the present magnitude-scale runs counter to both the articulatory and frequency sequence. The conclusion can hardly be avoided that *i·* is symbolically larger than ɪ because the length of the former vowel makes it 'sound' larger. The dark-versus-bright symbolism, on the other hand, seems to be patterned exclusively on an articulatory and frequency basis; the factors of vocalic length and size-of-mouth-opening evidently have nothing to do with the symbolic judgment of relative darkness, a situation which is entirely plausible.

Turning to the consonants, we find similar objectively mechanical factors at work to form the subjective scales. In judgments of small to large the sequence of articulatory position is consistently dental, labial, palatal (*t, p, k; d, b, g; n, m*). This symbolic scale further follows the objective sequence of voiceless to voiced (*t, d; p, b; k, g; s, z; c, j;* etc.). The consonants conform to a series of phonetic sequences with a fairly definite mathematical regularity. The scale separations between some of the consonantal magnitude-values are listed.[6]

Position:

	dental to labial		labial to palatal	
	t-p	.3253	*p-k*	.2243
	d-b	.3395	*b-g*	.1951

Voicing:

stops		fricatives		sibilants	
t-d	.3808	*θ-ð*	.4384	*tc-dj*	.5657
p-b	.3950	*f-v*	.4950	*c-j*	.6081
k-g	.3658				

[6] The reader should consult the consonantal magnitude-scale of Table XVII, which is more complete in having the consonantal responses of Group 3 (Experiment 1) grafted on the scale of Experiment 2A. The consonantal responses of only Group 3 were used because this group corresponded most closely in age and in number of subjects to the group of Experiment 2A.

The nasal consonants do not hold a consistent pattern in relation to the stop-consonants of their respective positions; but this apparent inconsistency is carried out in both of the symbolic scales. Although *m* (labial nasal) is both larger and darker than *b* (labial voiced stop), *n* (dental nasal) is both smaller and brighter than *d* (dental voiced stop).

The bright-to-dark symbolism is likewise objectively phonetic in its consonantal patterning. A voiceless-to-voiced sequence is adhered to, as in the small-to-large symbolism; apparently the voiced consonant, having more acoustic body than a voiceless sound, gives an impression of darkness as well as largeness. The articulatory sequence, on the basis of a bright-to-dark symbolism, in palatal, labial, dental—an order which is the reverse of that on the magnitude-scale. The palatal consonants, then, are bright but large; the dentals, on the other hand, are dark but small.

The symbolic darkness-scale is not complete enough for a presentation of mathematical relationships between consonants, as was furnished for the magnitude-scale. The pattern of symbolic darkness, as far as one can judge, does not show as thoroughgoing a mathematical regularity between the phonetic relations of consonants as that of symbolic magnitude. Yet when one compares the discrepancies of the consonants with those of the vowels (Tables XIII and XIV), the fact that the former indicate a less reliable index of symbolism makes it all the more surprising that they should be as consistently patterned as they are. Their symbolic ambiguity, in contrast to the vowels, reflects in a sense the embarrassment of acousticians confronted with the problem of making a physical analysis of consonants; if the symbolic judgments of phonetic sounds are based upon their objective properties, as these experiments seem to indicate, a subtlety and variability of objective composition will quite naturally be mirrored in a vagueness of symbolic reference.

The corrections for the variable of phonetic structure were computed from the formula

$$K = (Xe—Xc)\ (2)^{\frac{1}{2}}$$

K = the scale distance between the modal stimulus-value and the value of that stimulus under a particular condition of phonetic structure.

Xe = the experimental sigma-value corresponding to the experimental proportion of a stimulus-pair.

Xc = the sigma-value of the calculated proportion.

$2^{\frac{1}{2}}$ = a constant (the discriminal errors of the stimuli) used in the scale formula (*supra* p. 58).

An example of the procedure is given:

Stimulus Contrast	Xe	Xc	Xe—Xc
b-p	.6219	.2793	.3426
g-d	.3989	.3780	.0209*
gl-k	.5740	.5634	.0106
gr-m	.2045	.4034	—.1989
r-n	.5978	.5733	.0245
l-s	.3292	.2614	.0678
dj-h	.1332	.4287	—.2955

All of the consonant pairs in this illustration were given initially, accented, in three-syllable words (*báguli-páguli, dɔmali-gɔmali, glápuni-kápuni,* etc.). The median difference between the experimental and the calculated sigma-scores was taken as the normal value for this condition of phonetic structure. This value is starred in the above example. Using formula $K = (Xe—Xc)\ (2)^{\frac{1}{2}}$, we may conclude: Init., acc., 3-syl. word $= 0.0209\ (2)^{\frac{1}{2}} = 0.0296$. This procedure was applied to all pairs presented under the same conditions of phonetic structure. The calculated proportions employed in computing the discrepancies were in each case corrected by the structural condition under which the particular stimulus was given (Discrepancy = Pe—Pc, in which Pc is the proportion corresponding to Xc corrected by the sigma-value of K).

In spite of other indeterminate fluctuations disturbing the character

TABLE XV

INDIVIDUAL CORRECTIONS FOR PHONETIC STRUCTURE

When the syllabic structure of a condition is not expressed in the table, it is understood to be a two-syllable word.

Vowels

	Exper. 2A	Exper. 2B
K 1. Unacc., first syl.	—.4111	—.1293
K 2. " final	—.3379	—.1817
K 3. First syl., acc., final cons.	—.0202	—.0413
K 4. " " " cons. doublet	.0006	—.1127
K 5. " " "	.0823	—.0443
K 6. One syl. word	.1266	.0092
K 7. Final, acc.	.1848	.2800

Consonants

	Exper. 2A	Exper. 2B
K 1. Final, acc.	—.2437	—.2876
K 2. " unacc.	—.2353	—.4342
K 3. Initial, acc., final cons.	—.2333	—.0686
K 4. " " long final vowel	—.0535	—.0209
K 5. " unacc.	—.0423	—.1622
K 6. One syl. word, initial	—.0412	.0083
K 7. " " " final	—.0235	—.1273
K 8. Initial, acc., 3 syl. word	.0296	—.1397
K 9. " " cons. doublet in syl.	.0808	—.0870
K10. Medial, accent following	.0940	—.0908
K11. " " preceding	.1143	—.0358
K12. Initial, acc., long vowel following	.1584	.1863
K13. " " diphthong following	.2728	—.1988

of this variable of phonetic structure, the corrections tend to fall into a typological pattern (Tables XV and XVI). The vowel corrections follow essentially the same rank-order in both the magnitude- and the darkness-scales, but the consonants do not. The symbolic value of accenting the vowel is heavily in favor of largeness and darkness; the final position of the vowel likewise carries with it a large and dark symbolism. In the consonants, on the other hand, the final position is the weakest for symbolic magnitude and darkness. The structural pattern for the conson-

TABLE XVI

TYPOLOGICAL CORRECTIONS FOR PHONETIC STRUCTURE

Experiment 2A (Large vs. Small)

The scale-value of a typological correction is the mean of its individual corrections (Table XV). The individual conditions making up a type are given in parentheses.

Vowels	
KI (K1, K2) Unaccented	−.3653
KII (K3, K4, K5) First syl., acc.	.0256
KIII (K6) One syl. word	.1266
KIV (K7) Final, acc.	.1848
Consonants	
KI (K1, K2) Final	−.2403
KII (K3, K4, K5) Initial unemphatic	−.1011
KIII (K6, K7) One syl. word	−.0305
KIV (K8, K9) Initial neutral	.0499
KV (K10, K11) Medial	.1047
KVI (K12, K13) Initial emphatic	.2155

ants of magnitude-symbolism shows three types of initial position. Psychologically considered, these might be termed: (1) *initial unemphatic,* when the syllable containing the consonant is unaccented, or when the word ends in a consonant or a long vowel—conditions which might be interpreted as emphasizing some other part of the word than the initial syllable; (2) *initial neutral,* when the syllable is closed by a consonantal doublet, or when it is part of a three-syllable word; (3) *initial emphatic,* when the syllable containing the consonant is emphasized by having a long vowel or a diphthong. The medial position is symbolically larger than the initial neutral, but smaller than the initial emphatic.

Phonetic symbolism and English words. The data amassed for the magnitude-values of vowels, consonants, and phonetic structure were employed to determine whether meaningful English words reflected this type of phonetic symbolism. For this purpose a group of words were gathered from Roget's *Thesaurus;* all the words under the rubrics, Greatness, Smallness, Size, and Littleness were listed (Nos. 31, 32, 192, 193), and the repetitions, derivatives, and phrases were struck out. The resulting list,

containing almost five hundred words, included many terms which did not seem to belong clearly to the categories of denotative largeness and smallness. The list was then divided into the two denotative classes and given to 11 judges, who were asked to check the words that, in their

TABLE XVII

MAGNITUDE-SCALE VALUES UNDER CONDITIONS OF PHONETIC STRUCTURE

In accord with the procedure of arranging scale-sequence throughout this paper, the stimulus with the lowest scale-value is taken as the origin. The consonant-responses of Group 3, Exper. 1, have been grafted on the consonantal magnitude-scale of Exper. 2A.

Vowels

	KI	KII	KIII	KIV
ɪ	.0000	.3909	.4919	.5501
i	.2687	.6596	.7606	.8188
ei	1.0370	1.4279	1.5289	1.5871
ai	1.1029	1.4938	1.5948	1.6530
ä	1.1045	1.4954	1.5964	1.6546
yu	1.2521	1.6430	1.7440	1.8022
u	1.4061	1.7970	1.8980	1.9562
a	1.6544	2.0453	2.1463	2.2045
ɔ	1.7047	2.0956	2.1966	2.2548

Consonants

	KI	KII	KIII	KIV	KV	KVI
t	.0000	.1392	.2098	.2902	.3450	.4558
c	.3215	.4607	.5313	.6117	.6665	.7773
p	.3253	.4645	.5351	.6155	.6703	.7811
n	.3507	.4899	.5605	.6409	.6957	.8065
d	.3808	.5200	.5906	.6710	.7258	.8366
f	.3819	.5211	.5917	.6721	.7269	.8377
s	.4053	.5445	.6151	.6955	.7503	.8611
h	.5213	.6605	.7311	.8115	.8663	.9771
k	.5496	.6888	.7594	.8398	.8946	1.0054
tc	.5619	.7011	.7717	.8521	.9069	1.0177
θ	.6222	.7614	.8320	.9124	.9672	1.0780
b	.7203	.8595	.9301	1.0105	1.0653	1.1761
l	.7750	.9142	.9848	1.0652	1.1200	1.2308
v	.8769	1.0161	1.0867	1.1671	1.2219	1.3327
g	.9154	1.0546	1.1252	1.2056	1.2604	1.3712
j	.9296	1.0688	1.1394	1.2198	1.2746	1.3854
m	.9607	1.0999	1.1705	1.2509	1.3057	1.4165
ð	1.0606	1.1998	1.2704	1.3508	1.4056	1.5164
dj	1.1276	1.2668	1.3374	1.4178	1.4726	1.5834
r	1.1615	1.3007	1.3713	1.4517	1.5065	1.6173
z	1.2255	1.3647	1.4353	1.5157	1.5705	1.6813

opinion, did not conform in meaning to their classifications. The final list, taken from a majority vote of the judges, is printed in Tables XVIII and XIX.

A frequency table was made for the distribution of phonetic sounds in each of these meaningful categories; the values used were those of Table XVII, which includes the symbolic magnitude-values for vowels and for

consonants under the various conditions of phonetic structure.[7] This gave two independent approaches to the determination of symbolic magnitude in English words denoting largeness and smallness. It was necessary to construct a third frequency-table for the distribution of short vowels; the

TABLE XVIII

WORDS DENOTING SMALLNESS

abate	epitome	melt	portable	spare
abridge	evanescent	mere	powder	spark
animalcule		microbe	puny	speck
atom	faint	microcosm	puppet	spice
attenuate	few	midge		splinter
	flitter	miniature	rag	sprinkling
barely	fly	minikin	rare	squat
bit	fraction	minim	rather	stunted
bubble	fragment	minimum	reduce	subside
button		minnow	rudiment	subtract
	gleam	minor	runt	sup
chip	globule	minute		
chit	gnat	mite	scant	tag
close	grain	mitigate	scarce	tatter
contract	grub	modest	scrag	tenuous
corpuscle		modicum	scrap	tender
crumb	hardly	molecule	scrubby	thin
crumble		monad	scruple	tincture
	inch	morsel	seed	tinge
decimate	infinitesimal	mote	seldom	tiny
decline	iota	mouse	shade	tit
decrease	insect		shadow	tittle
depreciate		narrow	shaving	tomtit
descend	jot	near	sheer	touch
detail	just	nutshell	shiver	trifle
deteriorate			short	
diminish	languish	only	shred	urchin
diminutive	least		shrimp	
discount	less	paltry	shrunk	vanish
dole	light	paring	simple	
doll	Liliputian	part	sip	wane
dot	limited	passable	slender	waste
driblet	little	patch	slight	weak
drop	look	paucity	slip	wear
dumpy	low	pebble	sliver	weazen
dwarf		petty	smack	wee
	maggot	pigmy	small	whit
ebb	mannikin	pigwidgeon	snack	worm
elf	meagre	pin	snatch	wretched
eliminate	mean	pocket	snip	
embryo	mediocre	point	sop	

values used here were those of Experiment 1; but the variable of phonetic structure was not introduced, this variable having been computed from a

[7] The simplified set of typological conditions (Table XVI) was used in order to avoid unnecessary detail. The consonant doublets, *gl*, *gr*, and *br*, were not included in the table of frequency-values because their position on the magnitude-scale indicated that they did not behave as consonantal units.

TABLE XIX

WORDS DENOTING LARGENESS

absolute	entire	immense	perfect	strong
abundant	exceed	important	plenary	stupendous
amazing	excessive	increase	plump	sufficiency
ample	exorbitant	incredible	porpoise	supreme
arch	expand	indeed	portly	superlative
assemblage	expanse	intense	positive	surprising
astonishing	extent		pot	swollen
awful	extraordinary	lamentable	power	
	extravagant	large	precious	terrible
behemoth	extreme	leviathan	preeminent	thumping
best		load	preposterous	thundering
big	fabulous	lump	principal	tower
brawny	famously	lusty	prodigious	transcend
bulk	fat		profound	tremendous
burly	flagrant	magnificent	prominent	truth
bushel	fleshy	magnitude	proportions	
	flood	main	puffy	ultra
capacity	frightful	major		utter
cargo	full	mammoth	quantity	
chief	furious	many		vast
chubby		marked	rank	veriest
colossus	Gargantua	marvellous	remarkable	very
comprehensive	giant	mass	rich	violent
considerable	gigantic	might	roaring	volume
consummate	glaring	monster		
corporation	goodly	mound	several	well
corpulence	grave	mountain	shocking	whacking
crass	great	much	size	whale
Cyclops	gross	multitude	soar	whole
			sound	wholesale
deep	heap	noble	space	whopper
dimension	heavy	notable	stalwart	wide
dreadful	high	noteworthy	stark	wonder
	hippopotamus	number	stock	world
egregious	horrible		store	
elephant	huge	obese	stout	
emphatic	hulk	outrageous	strength	
enormous		overgrown	striking	

different experiment. The three indexes (Sf/f, in which S = the magnitude-value of a phonetic sound on the symbolic scale, and f = the frequency of a phonetic sound in English words) give the average symbolic magnitude of (1) long vowels and diphthongs, (2) consonants, and (3) short vowels. The values of the three indexes are as follows:

	Words Denoting Largeness	Words Denoting Smallness
(1) Long vowels and diphthongs	0.9832	0.9162
(2) Consonants	0.8476	0.8024
(3) Short vowels	1.3711	1.4072

From this table it must be admitted that the phonetic content of English words takes practically no account of magnitude-symbolism. The large/

small ratio of the three indexes is respectively 1.08, 1.06, and .97. It may be argued that English words do show, perhaps, a very slight regard for symbolic value, since the first two indexes are more reliable in taking account of the variable of phonetic structure; but this tendency is so minute as to be negligible. The actual distribution of sounds in the two denotative categories is fairly random, as far as phonetic symbolism is concerned.

These investigations lead us to several conclusions, which may be summed up as follows:

(1) Phonetic elements tend to be rigidly patterned on a non-linguistic symbolic scale. This rigidity of subjective patterning is evinced by the fairly consistent agreement between the three groups of Experiment 1 in respect to the sequence of phonetic stimuli on a symbolic scale as well as to the discriminal distances between them. The suggestions given by the apparent regularity of the discrepancies and by the consistent relations of objectively phonetic factors point to the same conclusion.

(2) The factor of age, as far as this investigation has gone, has little effect on the subjective scale, the slight variations in patterning being mere differences in the degree of consistency and of discriminal clarity. The child of nine years shows the same plan in his symbolic judgments as the adult.

(3) The basis of phonetic symbolism is fundamentally objective. The symbolic scale is constellated in accordance with such mechanical factors as position of articulation, acoustic resonance-frequency, size of oral cavity, vocalic length, consonantal voicing, and phonetic structure.

(4) Diverse types of phonetic patterns are formed by unlike symbolisms. This divergence of patterning is occasioned by a variety of objective factors entering into the symbolic judgments. The sequences of the tongue-position in articulation and of consonantal voicing appear to be basic factors in the patterns of magnitude- and darkness-symbolisms; but such factors as vocalic length and the size of the oral cavity are relevant only for judgments of magnitude.

(5) These symbolic judgments are not produced by linguistic associations. The investigation of the denotative categories of largeness and smallness in English showed practically no correlation between the meaningful and the symbolic distribution of speech sounds. The rigidity of the subjective scale would in itself preclude any such chance factor as linguistic association. The underlying objective rationale of the symbolic pattern would further tend to deny such an assumption, for it could hardly be maintained that the distribution of phonetic sounds in meaningful English words follows a mechanical and objective schema.

AN ACCESSORY STUDY OF "PHONETIC SYMBOLISM"

By MADISON BENTLEY and EDITH J. VARON, Cornell University

In 1929 Sapir published a brief study from which he concluded that certain vocalic and consonantal sounds have a definite symbolic significance unrelated to the associative and linguistic value commonly attached to words.[1] His method consisted in presenting two 'unfamiliar' words (*e.g. mal* and *mil*), to which a meaning (*e.g.* 'table') was arbitrarily attached. His subjects reported whether *mal* symbolized a larger or a smaller table when compared with the word *mil*. The results indicated that there was a tendency for vowels near the *a*-end of the vocalic scale to imply 'largeness' and for those near the *i*-end to imply 'smallness,' quite apart from linguistic uses of these sounds. A like reference was also discovered for certain consonants.

In the immediately preceding article in this JOURNAL S. S. Newman has, at the instance of Professor Sapir, again attacked the problem of phonetic symbolism, applying to his research a method of statistical treatment not used in the earlier study, and using a much larger body of material than the earlier study commanded.[2] Newman's results generally confirm Sapir's earlier conclusions, making the interpretation much more specific. Newman again makes phonetic symbolism apply differentially to 'large' and 'small' and adds also the contrasting pair 'bright' and 'dark.' The distribution of his vowel-sounds and consonant-sounds upon the scale of magnitude (large-small) and the scale of brilliance (dark-light) leads him to believe that it is determined by such mechanical factors as position of the tongue in articulation, resonance within the mouth, the size of the oral cavity, and length of the vowel sound. Some differences in these factors with respect to their influence upon the two scales are thought to be significant.

Upon the appearance of the first article, we were led to undertake a bit of experimentation upon the subject, partly to adapt the method to the psychological laboratory and partly to discover how far "phonetic

* Accepted for publication October 1, 1931.

[1] E. Sapir, A study in phonetic symbolism, *J. Exper. Psychol.*, 12, 1929, 225-239.

[2] S. S. Newman, Further experiments in phonetic symbolism, this JOURNAL, 45, 1933, 53-75.

symbolism" was actually inherent in the sounds used and how far it arbitrarily laid hold of variety in the make-up of the non-linguistic 'words' and turned them perforce into degrees of 'magnitude.' We used a small number of students trained in research, and we devised various means for extracting size or magnitude, without hinting at it, from the presented sounds. Furthermore, we wished to discover—in the event of 'a more or less,' 'a this or that'—whether such an intimation of degree or of contrasting quality pertained specifically to spatial determinations. In certain series, therefore, we added to *size* the nine categories of *angularity, foolishness, endurance, liquidity, sentimental attachment, motion, noisiness, solidity,* and *strength.*

Method I. (One category word: 'large.') Forty series of 10 nonsense syllables each (vowel between two variable consonants) were made up and presented to each of three *O*s (*D, G,* and *L*).

The *O*s were graduate students trained in the procedures of the experimental laboratory. They knew therefore how to observe under prescribed instructions and how to make relevant and precise reports. Each series contained either two or three familiar words and seven or eight non-linguistic (n-l) sounds. Of the latter, two were *critical*; *i.e.* they contained vowels taken from near the ends of Sapir's vocalic scales. One vowel was *a* (Ger. M*a*nn), the other *i* (Fr. f*i*ni). The completing consonants were varied. The critical sounds always appeared between 'nonsense' sounds. In the first 20 series they were separated from each other; in the last 20 series they directly followed each other, as a possible enhancement of the 'contrasting' vowels. *O* was screened from *E,* that mouth-setting, posture, or gesture might not be effective.

O was instructed to respond promptly with the first sound that came after hearing a given sound pronounced by *E.*[3] Since rhymes and assonances were at first frequently given in response, *O* was presently instructed either to give a synonym or otherwise to express the meaning of the heard sound, but to avoid rhymes and similar-sounding words. The forty series were three times given (in changed order) to each *O* (120 series $\times$ 10 sounds each $\times$ 3 *O*s $=$ 3600 sounds altogether). Sometimes we found the sound completed to form a word (*saʃ*—Sappho); sometimes an onomatopoeic meaning came (*zin*—noise of a flying missile; *vub*—a frog

[3] Although one experimenter (Varon) uttered all the sounds used in this study, the precise and consistent character of the non-linguistic sounds proved to be difficult. As Dr. Sapir has urged in correspondence, the common use by him and us of identical phonetic symbols offers no guarantee that the same sounds were actually employed. We suggested master-records on phonographic disks; but the matter could not be arranged. There remains the possibility, then, that certain differences between his results (together with Newman's) and ours may be due in part to unlikenesses in the actual sounds uttered. Of course the Sapir experiments have the advantage of expert knowledge of phonetic quality and quantity. At the same time, we must see that, however unfaithful our sounds may have been to the high phonetic standards set, we have without doubt attained such differences in the "mechanical factors" assumed by Newman (*supra* p. 61) to be operative as should make our results as well as his significant for the problem of "phonetic symbolism."

sound; *zuv*—a soft, gentle something), and sometimes a near-lying word came (*kir* —care).

No responses definitely indicated, however, that such a factor as phonetic symbolization was present, at least in a degree that led to a response-sound that suggested size or any determination of magnitude. Of course this negative outcome does not imply that magnitudes and differences of magnitude might not be forced from the sounds under other conditions. 'Feeling significances' (Sapir) might be potentially at hand but too weak to compete with 'reproductive tendencies' (whatever these theoretical conditions of vocalization may be!) under our conditions of presentation. The results do suggest, however, that such sounds as we have employed do not *spontaneously* lead to other sounds (under the common conditions of 'free association') which denote or hint at spatial magnitude or its qualifications.

Method II. (10 category-words, each followed by an n-l sound.) Here we extended our search to the 10 categories named above. The method approached one step nearer that of Sapir (first article), but still preserved notable differences.

The general procedure was to sound one of the 10 category-words (angular, foolish, large, lasting, liquid, loving, moving, noisy, solid or strong) and then to sound a non-linguistic (n-l) sound with the instruction to report whether this sound implied some *degree* of the first or of its opposite (*e.g.* large or small). If it did, the report was to be 'slightly,' 'moderately,' or 'very;' if it did not, the report was to be 'unrelated.' Thus the second sound in each pair was non-linguistic (n-l). Choice was made here from a list of 50 monosyllables. For constructing these the vowel *e* (Eng. met) was added to the *a* and *i* used under Method I. Sometimes the initial consonantal sound was kept constant (*e.g.* gam, gem, gim), thus providing an opportunity to compare results with a single context for unlike vowels. Each series of ten pairs (*e.g.* for a pair, angular-kig) contained 4 *a*-sounds, 4 *i*-sounds, and 2 *e*-sounds. The category-words and the n-l sounds were combined in a haphazard order. As each category-word was given with each n-l sound, 500 combinations were presented to each *O* (10 words × 50 following-n-l-sounds × 3 *O*s = 1500 instances in all). The *O*s were *D*, *G*, and *L* as before.

Of the 1500 reports under this method 450 indicated some kind of relation (called *positive* report) between the category word and the n-l sound following it in each pair.

Agreements and disagreements among the *positive* reports stand as follows for the 450 instances;

Agreement			*Disagreement* (in direction)	Total
Reported by 1 *O*	2 *O*s	3 *O*s		
202	174	18	56	450

Thus well above two-fifths (202) of the cases of positive relation were reports made by a single *O*. Two *O*s agreed upon approximately four-ninths (192) of the cases; all three *O*s agreed only 18 times. Disagreement as to amount or direction of the relation was shown in 56 cases. These disagreements are illustrated by *lel,* reported as 'moderately large' (*D*), and as 'moderately small' (*G*); *wim* reported as 'slightly weak' (*G*), and as 'equally weak and strong' (*D*).

A little less than one-third of the whole number of reports (450:1500) indicate, then, that, to the n-l sounds used, the quality expressed in the category-words was applied by one or more of our *O*s, under the conditions imposed by our experiments. Upon most (960) of the 1050 'unrelated' or *negative* reports, two of the three *O*s agree. This high agreement upon the *want of relation* may be as significant as the *positive* reports. It seems to imply that a good many of the total n-l sounds used (*i.e.* the total 'nonsense syllable') symbolically represent *no* qualification of the categories used.

As regards our positive reports, an analysis of the results shows that a large number of our 50 n-l sounds led to some degree of relationship where each one of our 10 categories was used. With respect to the degree of relationship (slightly, moderate, very), few (only 8 altogether) 'very' reports were made. Nearly all the reports alleged a 'slight' relationship. Of all 10 categories, the fewest positive reports were given with the meaning *solid*. Some notable differences with respect to the vowel of the n-l sound were observable. Thus the positive 'angular' reports were chiefly returned with *a*- and *e*-sounds, seldom with an *i*-sound. The 'solid' reports seldom came with an *e*-sound or the 'strong' reports with *a* and *e*. The total number of positive reports is distributed as follows among the 10 categories; angular 35, foolish 48, large 32, lasting 42, liquid 41, loving 41, moving 34, noisy 32, solid 23, and strong 35. It is possible, therefore, to report a relation (of more or less) over a wide range of diverse qualities.

Method III. (5 categories and their opposites,with n-l sounds in pairs.) As a still closer approach to the original method of Sapir, we next followed our category-word with *two* n-l sounds. The *O* was instructed to report which (if either) of the two n-l sounds more closely approximated the meaning of the category-word just given.

Five categories were used; *foolish, angular, liquid, large,* and *strong.* First, one of these category-words was pronounced (*e.g.* 'foolish'), then (at a suitable interval) two n-l sounds (*e.g.* 'zat,' 'fim') in quick succession and with an even inflection and intonation. The sounds *o* (go) and *u* (tufa) were added in this series. We considered that, although our results would be few and not admissible

to statistical treatment, they would be obtained with trained and practiced *O*s (the 3 *O*s of Methods I and II), working under evener and more constant conditions than could well be commanded in a group, and that they might give hints of selectiveness in the phonetic scales as Sapir and Newman have observed with fairly large groups of *S*s. Twenty combinations with each category-word were made, in both temporal orders for the n-l sounds.

In the 300 pairings (5 x 20 x 3 *O*s), some degree of relationship was reported 165 times and for all 5 categories. In only 7 cases was the degree reported as 'very,' and as 'moderate' only 17 times. All the others are 'slight.' Comments by all *O*s bear out the suspicion that the relation was usually forced and that, when it was reportable, the difference in 'size,' 'liquidity,' etc., as between 'zat' and 'riv,' 'lel' and 'duf,' etc., was both doubtful and obscure. The case is different with Newman, who asked his *S*s whether this or that n-l sound was the larger or the smaller 'horse,' and so on. In his case the two sounds were arbitrarily given the meaning 'horse' and the larger (or smaller) arbitrary 'horse-sound' was then checked. Our *O*s were left free to discover that either or neither of the n-l sounds applied to size, strength, and the other category-words. In both studies it is obvious that the formal and the occasional instructions are weighted—albeit by different devices—to force a meaning into an n-l sound and then to suggest that this meaning admitted of degree. Most of the Sapir-Newman results bear upon the "magnitude-pattern" forced upon names, activities and adjectival qualities. Newman added (Experiment II) a "darkness pattern." Our employment of a larger number of "patterns" (category-words) makes it again obvious that *O*s may be forced to report differentially upon degree in many other contexts as well. It is strikingly attested also by the fact that our *O*s reported 21 *opposites* for 'large,' 'liquid,' and 'strong;' *i.e.* certain non-linguistics (n-l) were reported as 'small' (6), as 'solid' (7), and as 'weak' (8), although these words had not been prescribed. Of these opposites, 14 were *e*- and *i*-sounds, and 6 were *a*-, *o*-, and *u*-sounds. But 'liquidity' and 'strength' favored the *i*-sounds quite as much as size did.

Method IV. (Group experiment: 3 categories and their opposites; partially forced meanings.) We now used the 'arbitrary meaning' device of Sapir. Twenty-six members of a class doing drill-experiments in psychology were used. The general setting was experimental, for a part of the laboratory-period was devoted to our observations. In order to enhance the experimental setting, the instruction as given by Sapir was somewhat modified.

Sapir found that his exercise with high school students "seemed to be enjoyed by the great majority of his subjects" (high-school students) "as a rather interesting

game."[4] Although his conditions and instructions were not quite clear, he seems to have prearranged that two sounds, about to be uttered, were to be given an arbitrary meaning (*e.g. mal* and *mil* were to be given the meaning 'table'). Then when these sounds were given, the *S* checked in a column marked "large" if the first of the sounds "symbolized the larger reference," and in a column marked "small" if the first sound symbolized the smaller reference. Whether 'copying' was controlled among the 500 *S*s does not appear. To eliminate errors of time-order, in our experiments, and to make sure that the cumulative results of *O*'s own checking should not influence each succeeding check-mark, the written result was immediately removed from his view as soon as it was finished. In column-checking a common form of error from occasional instruction is to even the number of entries in both (or all) the columns in the serial run. Whether the children observed Sapir's face when he uttered the sounds, does not appear. The open-mouth continental *a* might be quite enough to swing the *a*-words to the 'large' column. That is one reason for our invariable use of a screen. Another possible source of degree-difference in n-l sounds emanates from the hearer. Sapir dwells upon this possibility, factoring it into auditory and kinaesthetic components. Unfortunately, this source was not sufficiently controlled, either in his and Newman's investigations nor in ours. How much all our *O*s used their vocalizing apparatus—either for incipient sounds or for 'gesturing'—no one knows. Five minutes' attention to the strains and postures incident to making brief vocal sounds will convince one that there is here plenty of material which, given a linguistic turn, will easily absorb 'degrees,' not only of magnitude but also of many other qualifications. We used 36 n-l paired combinations. First, the four sounds *fam, jaf, maf,* and *vag* were paired with *fim, jif, miv,* and *vig*. Thus 'contrasting' vowels were systematically distributed and combined. In the second place, we added sounds to pair off all the vowels among themselves (as in Method III). Finally, consonantal variations were used with each vowel (*e.g.* bav-pav, bav-pag, pav-pag, etc.) to discover the specific effect of the consonants. Haphazard order was followed throughout. There were 4 series of 9 sound-pairs each. Three categories (*angular, large, hard*) and their common opposites (*round, small, soft*) were used with all 36 n-l pairs. The order of utterance in each case was: (1) a pair of n-l; (2) object-, action-, or quality-word; (3) category-word. An instance is (1) fam-fim; (2) cabin; (3) which is the larger cabin? The vocal presentation had been preceded by the general instruction: "I shall utter two sounds (*e.g.* fam-fim), then an arbitrary meaning which you are to apply to them (*e.g.* cabin), then ask you to compare the two sounds, if you can, with respect to some attribute (*e.g.* size)." Here we departed from Sapir's procedure in leaving the possibility that the two sounds might not be comparable with respect to the attribute. We wished to force the comparison as little as possible.

Our tables show, as the Chicago studies had shown, a persistent inequality in the 'largeness' of the various vowel-sounds. Here are the results from a paired comparison of four *a*-sounds and four *i*-sounds with the arbitrary meaning 'table.'

[4] *Op. cit.*, 229.

Object = Table (large and small)

	A-sounds (fam, jaf, mav, vag)	I-sounds (fim, gif, miv, vig)	totals
'larger'	176	46	222
'smaller'	41	189	230
totals	217	235	452

The *a*-sounds are *larger* than the *i*-sounds (*table*) in the approximate ratio of 4:1. Reading down each column, the large-small ratio for *a*-sounds (table) is also approximately 4:1. The reverse relation holds also for the *i*-sounds. While calculated reliabilities would have little significance for our small number of observations and of observers, the internal consistency is striking.

It is worth nothing that, in size-of-table, the *a*-sounds are conspicuously *more* in number of 'large' reports than the *i*-sounds, and conspicuously *less* in number of 'small' reports *in every one of the sound-pairs compared.* With respect to size, then, *a* (as in Ger. Mann) is consistently *larger* (and less small) than *i* (as in Fr. fini).

We may add similar results with the same paired sounds when the arbitrary object is 'table' (angular and round) and is 'chair' (hard and soft).

Object = Table (angular and round)

	A-sounds (same as above)	I-sounds (same as above)	totals
'more angular'	65	193	258
'rounder'	199	55	254
totals	264	248	512

Object = Chair (hard and soft)

	A-sounds (same as above)	I-sounds (same as above)	totals
'harder'	88	164	252
'softer'	162	88	250
totals	250	252	502

As regards angularity we find again a decided difference as between the two n-l sounds. For both sounds, the ratios for angularity and round stand 1:3 and 3:1. Again the totals for the two sounds are about the same (264, 248.)

With respect to hardness, *a* is softer (and less hard) than *i*. An unlikeness in clarity of difference—with respect to the three qualities—appears in the three approximate ratios, 4:1 (size), 3:1 (roundness), and 2:1 (softness). That the *e* used (as in Eng. m*e*t) stands, in the respects noted, near our *i* is suggested by the fact that *fam* is reported larger than *fem* (table) and softer (and less hard) than *fem* (chair) by fairly large ratios (cf. Newman's quantitative results).

It appears at once that the *positive* reports form only a moderate fraction of the whole number. We have omitted all unrelated reports, as well as reports of *same* and *doubtful*.

Although Sapir and Newman are not clear upon the point, they seem to say that their method of forced meaning led only and always to the report of *unlike degree*. Our method left *O* free to note or to fail to note the applicability of the category to the comparison in question. The forcing instruction, in such cases, is bound to increase the number of *positive* reports, but it may well affect validity also. One wonders whether Sapir's *S*s may not, in 'playing their game,' have soon learned under forcing to apply a half-formulated rule with respect to sounds and magnitudes. Every one familiar with the conduct of psychophysical methods knows how readily even highly trained *O*s pick up devices which relieve them of making *bona fide* comparisons.

Of course the discovery that arbitrary meanings and forced choices may lead invariably to a comparative report of, say, magnitude does not prove that the 'magnitude' dimension came out of the n-l sound in the form of a 'phonetic symbol.' It only shows that, under the conditions, the *S*s apply, with more or less consistency, *some sort of difference* in the two n-l sounds to the problem proposed by the experimenter. In our own experiments we have found that the qualifications suggested by 'arbitrary meanings' come out in proportion to the amount of forcing. 'Free association' brought only a few conventional responses, either through a linguistic similar (mel-smell), a completion (saf-sappho), or an obvious onomatopoeia (bah-baa!). No real 'phonetic symbolism' in Sapir's sense appeared. The positive report did appear when *degree in some scale* was suggested and prescribed.

Sapir's fascinating problem lies on the border-line between linguistic material and non-symbolic sounds. It is natural that a student of languages and of phonetics should incline the problem toward his own field. The articles constantly refer to 'words' even where non-linguistic sounds (n-l, in our own shorthand) were used. It may be instructive, therefore, to drop for the moment this context and to regard the sounds as auditory or other objects of perception. This would be the psychologist's natural inclination. Then it would appear that the simplest and best-ordered series of

sounds (the tonal series) is a one-dimensional series, proceeding from low, grave, voluminous, dull, and weak members, at the one end, to high, acute, small, bright, and strong members, at the other. After decades of conjecture and experiment upon the possible 'associative' character of certain of these qualifications, it is generally agreed among psychologists that some such set of aspects is actually attributive and therefore inherent in the tones and that they gradually change from one end of the series to the other. As everyone knows, the effectiveness and significance of speech, music, and natural sounds constantly exemplify—and often voluntarily use—these attributive qualifications of the simpler sounds. Intensity is another instructive attribute. It is capable of fine gradation and may directly (*i.e.* without symbolization) be apprehended as strength. Once more, there is in all these attributive qualifications a principle of surrogative function by way of which any one may stand for any other and may be interchangeably used. Thus a decided intensive difference will readily adopt the context of size or of gravity and so stand for a 'contrast' in volume or in pitch. In fact, it requires long and rigorous training to distinguish modifications in pitch, volume, dullness and intensity.[5]

When we turn to the simpler noises, we find virtually the same set of attributes; though there is probably no actual identity, quality for quality, in the two series. If then we proceed to those complexer sounds which form the basis of linguistic communication, we find again the same sort of distinguishing characteristics. But here the case is considerably complicated by hundreds of diverse fusions of individual sounds. Nevertheless, it is a commonplace to the students of language and of music that the simpler vocal sounds partake of the general nature and constitution of their fused and temporally colligated members (compare tone and note, note and vowel, vowel and larger phonetic unit).

As it happens, Sapir lighted upon a qualification for 'phonetic symbolism' which is exceedingly prominent in the simple tonal system; namely, voluminousness (magnitude), and Newman has added a second, namely, brightness (dull and bright). Possibly no other choice would have turned out to be so significant. The positions in the tonal scale from which the main ingredients of the vocalic sounds are drawn well illustrate obvious spacings with regard both to 'magnitude' and to 'brightness.'

Now these vocalic characteristics, and others like them in consonantal elements, pertain to the sounds *as sounds*. They attain a 'phonetic' or 'symbolic' signification only when we transfer them to other contexts. It

[5] Cf. R. H. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, this JOURNAL, 42, 1930, 519-543.

appears that they have not generally been useful on the meaning side of language, albeit they possess high aesthetic value both in speech and in music.

It does appear from these studies that certain tonal, vocalic, and consonantal characteristics, which likewise bear a fixed relation to physical sources of sound and also to the human productive mechanisms (position of tongue, resonance, size of oral cavity, etc.), as Newman and Sapir have pointed out, are so well graduated as to be usable when forced by artifice into the representation of size and of various other concepts and categories as well. Nevertheless, there seems to be insufficient evidence that these graded attributes of sound carry in their own right (so to say) a symbolic reference. Newman discovered,[6] as we might expect, that linguistic usage derives its verbal and syntactical meanings (true symbolization) from other sources. The case is probably the same for colors, where also we have heard a good deal of 'symbol' and 'fixed emotive significance.' Even gustatory and tactual qualities are useful indicators of many things, but chiefly by an adventitious overlay of reference upon qualitative specificity.

As a demonstration of the use of attributive characteristics of simple vocal and instrumental sounds in discriminations of the sort exemplified by Sapir and Newman, we presented to an introductory class in psychology (327 university students) 5 pairs of n-l sounds, three of them vocal (*fam-jif, vig-vam* and *miv-vag*) and two instrumental, the first a succession of simple tones from tuning forks (512 vs. and 2304 vs.) and the other pair two fairly simple noises of the explosive sort (the first produced by the thud of a padded mallet on heavy wood, the other the crack of a small pith-hammer upon a wooden surface).

The following instruction preceded the giving of the sound-pairs. "I shall utter two sounds in succession from behind this screen, first warning you with a 'ready.' As you hear the two sounds, consider which of them appears (just as a sound) *larger* (*lighter, softer, smaller*) than the other." The first vocal pair was compared for largeness, the second for lightness (as contrasted with darkness), and the third for softness. The two tuning-fork tones were compared for largeness and the two noises for smallness. The results follow.

Reported	First	Second	Neither	Totals
fam-jif ('larger')	165	83	79	327
vig-vam ('lighter')	240	62	25	327
miv-vag ('softer')	184	72	71	327
tones ('larger')	265	38	24	327
noises ('smaller')	15	304	8	327
	869	559	207	1635

[6] *Supra* pp. 71-75.

It appears, therefore, that untrained observers find it possible to distinguish these characteristics in n-l vocal and instrumental sounds without any such linguistic reference as Sapir and Newman used. We have no reason to believe that there was involved in these comparisons anything like a 'feeling significance' or a 'symbolic reference.' The comparisons seem to have been based directly upon inherent differences within the sounds themselves. It is notable that the greatest difference between the first and the second sounds (as implied in the distribution of the reports) occurs in the *simplest* sounds, where the lower tone is reported 'larger' than the higher in a ratio of approximately 7:1 and the higher noise is reported 'smaller' than the lower in a ratio of approximately 20:1.

Again, 'magnitude' (and perhaps other graded qualities) may find a direct means of representation in the immediate visual perception of an articulating organism. The open mouth, the stretched jaw, the tense lip, all doubtless carry spatial and dynamic meanings which immediately enter into the specific characterization of the vocalized sound as heard.

Finally, the act of sound-production itself, which includes the feel of the tongue, lips, jaw, chest, and other parts of the body, contributes its large share to the sound as heard. As the context is favorable, this tactual and kinaesthetic component, variable in many directions and through wide ranges, probably serves in quite essential ways to convey its own meaning.

In all these cases, the materials serve, not by any inherent power of specific symbolization but rather as a graded and labile system, to inform the occasion—whether linguistic or not—in some appropriate and significant manner.

Psychology is deeply indebted to these phonetic studies for suggesting a number of interesting and significant problems which are now ripe for further research.

A QUALITATIVE AND QUANTITATIVE ANALYSIS OF A MENTAL TEST

By MARTIN L. REYMERT, Mooseheart Laboratory for Child Research, and MILDRED L. HARTMAN, Antioch College

Numerous psychologists have recognized the possibility of the mental test movement losing itself in purely statistical refinement, while the qualitative aspects of the material measured are often entirely neglected. As early as 1914 Whipple says:

> Those who see in mental tests nothing but statistics of performance, and who contend that in the absence of introspection it is impossible to get any information upon mental content, have failed entirely to appreciate how significant for qualitative exploration are tests when handled by a competent investigator who puts less emphasis upon mere accumulation of figures than upon the patient search for qualitative aspects of mental life implicit in the outcome of his investigation.[1]

Mateer, in 1917, voiced similar opinions,[2] and more recently Crane has said:

> The data secured by psychometric technique are of immense importance to the clinical psychologist, as representing measures of traits, but there their contribution ends.[3]

With the rapid development of the test movement and with the present day stress on the quantitative psychology of individual differences, it is but natural that refinement of statistical method and creation of new tests have been of major interest. To be sure, leaders in the field have recently been stressing the necessity of a more penetrating analysis of the tests themselves, but hitherto this analysis has been confined almost exclusively to quantitative exploration. The stress upon quality rather than quantity as well as upon 'units of experience' or 'series of events' has received a great impetus from the more recent movements in psychology, as represented by the *Gestaltpsychology* of Köhler and Koffka, the *Ganzheitspsychologie* of Krueger, the *Personalismus* of Stern, the *Two Factor Theory* of Spearman,

* Accepted for publication July 20, 1932. This paper was read in part at the Ninth International Congress of Psychology. The experimental work was done at Wittenberg College.

[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, 2.

[2] F. Mateer, Some criteria for the evaluation of mental tests and test series, *Mental Hygiene*, 1, 1917, 241.

[3] H. W. Crane, The limitations of psychometrics in clinical practice, *J. Abnor. & Soc. Psychol.*, 26, 1931, 199.

as well as from newer systematic attempts in this country, (Hollingworth, Bentley, and others).

Reymert's discussion of such a tendency,[4] and the present paper offers an account of a rather intensive qualitative and quantitative study of the well-known Knox Cube Test.[5]

Method and procedure. The material for this test consists of 5 cubes of the same size and color. Four of the cubes are placed in front of *S*, separated by intervals of 2 in. *E* then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per sec.) always beginning with the cube at the child's left hand (*E*'s right if he is facing the child). *E* then lays the fifth cube down in front of the child equidistant between the third and fourth cube, but nearer the child, and says: "Do that."[6]

The series, consisting of 12 lines lettered from A to L, increases in difficulty supposedly forming a scale. A record of the number of lines passed or failed is kept. The examiner always continues with at least 3 lines after the child fails, and in many cases with more than 3 lines if there seems to be any possible chance of the child's succeeding in additional lines.

Subjects. We limited ourselves to an intensive study of 50 children and 100 adults. The children ranged in age from 13-14 yr. and the adults from 21-30 yr. The *S*s were tested at 2-week intervals, the children 8 times and the adults 5 times.

In addition to these results, other information was secured. *E* knew the children intimately and besides her knowledge of them gained from that source she gave them the following tests: "Silent Reading Test" (Gates), "The Multi-Mental Scale" (McCall), "The Standard Composition Test" (Hudleson), "Standardized General Survey Arithmetic Scale" (Monroe), "Intelligence Test," Form A and Form B (Terman), "Standard Reasoning Test in Arithmetic" (Monroe), and "Imagination Test" (Rorschach).

In retesting the children with the Knox Cube Test care was taken to work as nearly as possible at the same hour of the day as that of the first sittings. The tests were conducted in a separate room where the child was free from all disturbances, with no one but *E* present. The children, with very few exceptions, apparently enjoyed the test.

RESULTS

(1) Reliability of the test with children. It is apparent from Table I that the reliability of the Knox Cube Test for a group of 13-14 year old children is extremely low. The intercorrelations in the table were determined by the Pearson product moment method. Although the probably

[4] M. L. Reymert, Kvalitative Faktorer I Mental-Maalninger (Qualitative Factors in Mental Measurements), *Arkiv for psykolgi och pedagogik*, 1928, 27-38; Personal equation in motor capacities, *Scand. Scient. Rev.*, 2, 1923, 177-222.

[5] H. A. Knox, A scale based on the work at Ellis Island for estimating mental defect, *J. Amer. Med. Assoc.*, 62, 1914, 742.

[6] This is the procedure used by R. Pintner and D. G. Patterson, *A Scale of Performance Tests*, 1925, 68.

errors of the individual coefficients are rather high, the consistent lowness of the coefficients reveal definitely the very low reliability of the test. The coefficients vary from 0.12 (correlation between first and seventh and third and seventh trials) to 0.58 (correlation between second and third trials). The average intercorrelation is only 0.36. If the first trial is considered as practice, the average intercorrelation of the remaining trials is 0.40. Since the first trial of the test is often used as a measure of an individual's ability, the question arises as to the reliability of this trial.

The data show that performance in the first trial correlates, on the average, the least with performance in the other trials. The coefficient is 0.27 and for the other trials it varies from 0.28 to 0.43. This indicates that the

TABLE I

INTERCORRELATIONS, MEANS, AND STANDARD DEVIATIONS OF SCORES OF 50 CHILDREN ON SUCCESSIVE TRIALS OF THE KNOX CUBE TEST

Trial	1	2	3	4	5	6	7	8
2	.29							
3	.26	.58						
4	.20	.51	.39					
5	.23	.52	.34	.48				
6	.31	.40	.39	.56	.43			
7	.12	.29	.12	.45	.26	.50		
8	.48	.40	.22	.44	.41	.41	.21	
Av. r of each trial	.27	.43	.33	.43	.38	.43	.28	.37
Av. when Trial 1 is excluded		.45	.34	.47	.41	.45	.31	.35
Means	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92
S. D.	1.45	1.35	1.25	1.17	1.36	1.10	1.26	1.26

Av. r is 0.364; av. when Trial 1 is excluded is 0.396.

first trial is not a true index of the individual's ability for this task and that a standard performance is not obtained until a rather late trial, if ever. The correlation, between the first and the second trial is 0.29 for children, and 0.42 for adults.

To determine whether a reasonable reliability could be obtained by increasing the number of items in the test, the values in Table II were calculated by means of the Spearman-Brown formula. The table shows that in order to attain a reliability of 0.90, which is a reliability that should be demanded of a test used for individual measurement,[7] the number of

[7] A similar idea is advanced by H. A. Edgerton and H. A. Toops when they make the statement that "the authors propose, for purposes of discussion, that no intercorrelation table should ordinarily be presented unless based on tests or units (those on which the intercorrelations are based) which have at least a reliability coefficient of 0.90, unless the reliability coefficients are given, or unless the nature of the experiment precludes the possibility of such reliability being attained." (A table for predicting the validity and reliability coefficients of a test when lengthened, *J. Educ. Res.*, 1928, 225.)

items in the test would need to be increased 25 times. The administration of such a test would require an unreasonable amount of time and would therefore prove a prohibitive task.

On the basis of the average correlation of a trial with each of the other trials, Table I shows that Trial 1 is the most unreliable of the eight trials; excluding Trial 1, the averages indicate no improvement in reliability with successive trials. If we consider Trial 1 a practice test and take Trial 2 as a measure of the child's ability, then the situation is improved somewhat, but the reliability of the test is still very low.

The average of the correlations between Trial 2 and each successive trial is 0.45. Lengthening the test in number of items for Trial 2 would improve the reliability more than it would in the case of trial one, but even then, as Table III shows, a lengthening of 11 times would be required to attain a reliability of 0.90. Since the average intercorrelation of all trials

TABLE II

PREDICTED RELIABILITY OF TRIAL 1 IF THE TEST WERE LENGTHENED
(Av. correlation of Trial 1 with other trials = 0.27)

	Times lengthened						
	2	3	4	5	10	15	25
Predicted r	.42	.53	.60	.65	.79	.85	.90

is only 0.36 and even the average intercorrelation of the last 7 trials is only 0.40, we would obtain even poorer results by using these values as the reliability coefficients.

Since we have the scores on the test for 8 trials we can actually determine what would be the reliability of combined scores for 2, 3, or 4 trials and in this way check some of the values predicted by means of the Spearman-Brown formula.

The correlation between the sum of the scores on Trials 2 and 3 and the sum on Trials 4 and 5 was determined.[8] This coefficient is 0.58. If the number of items in Trial 2 were doubled a reliability coefficient of 0.62 would be obtained. The correlation between the sum of the scores on the first 4 trials and the sum of the scores on the last 4 trials is 0.68; according to the Spearman-Brown formula, lengthening Trial 2 four times will give a reliability of 0.77.

The reason for the discrepancy between the predicted and the empirical result is easily discovered. The assumptions underlying the Spearman-Brown formula are that the standard deviations of the successive tests are all equal and that the reliability coefficient used for prediction is equal to

[8] The general formula for determining the correlation between weighted sums is given by T. L. Kelley, *Statistical Method*, 1924.

the average of all the possible intercorrelation coefficients. The coefficient 0.45, used as the reliability coefficient of the second trial in predicting the values of Table III, is 0.09 larger than the average of all the possible intercorrelation coefficients which is 0.36. If this latter value is used as the initial reliability from which to predict the reliability of a test of four times the original length, the predicted value is 0.69 which is only 0.01 larger than the actual correlation between the sum of the scores in the first 4 trials and the sum of the scores on the last 4 trials.

It may be concluded that neither the predicted results nor the empirical results give evidence that satisfactory reliability might be obtained for a reasonable length of the test.

When scores are used for individual diagnosis, the probable error of a score is a more significant measure of reliability than is the reliability

TABLE III

PREDICTED RELIABILITY OF TRIAL 2 IF THE TEST WERE LENGTHENED

(Av. correlation of Trial 2 with later trials=0.45)

	Times lengthened					
	2	3	4	5	10	11
Predicted r	.62	.71	.77	.80	.89	.90

coefficient. The formula for probable error of a raw score which was used is $P.E._{r.s.} = 0.6745\ S.D.\ (1-r_{12}^2)^{\frac{1}{2}}$. The probable error of a raw score based on the standard deviation and reliability coefficient of Trial 1 and the probable error of a raw score based on the standard deviation and reliability coefficient of Trial 2 are:

Trial	$P.E._{r.s.}$	S.D.	Reliability Coefficient
1	.94	1.45	.27
2	.81	1.35	.45

The smaller probable error is 0.8. The meaning of this probable error is that if a score of 6 is made on one trial, there is a one to one chance that the score made on another trial will lie between 5.2 and 6.8, and a chance of about 4 to 1 that the score will lie between 4.4 and 7.6.

Pintner gives the average number of lines passed on the Knox Cube Test by groups of children of various ages as follows:[9]

	Age in Years																
	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Adult
Median No. Correct	1	1	2	4	5	6	6	6	7	7	7	8	7	8	9	10	8

[9] *Op. cit.*, 137, f.n. 6.

If these averages are used as age-norms, then a score of 6 corresponds approximately to the age of 10 yr. From the explanation of the probable error above it is plain that one-half of the individuals classified as 10 yr. of age on one trial would on another trial probably be classified below 8

TABLE IV

INTERCORRELATIONS, MEANS, AND STANDARD DEVIATIONS OF SCORES OF 100 ADULTS ON SUCCESSIVE TRIALS OF THE KNOX CUBE TEST

Trial	1	2	3	4	5
2	.42				
3	.38	.48			
4	.36	.44	.55		
5	.33	.59	.54	.54	
Av. r of each trial	.37	.48	.49	.47	.50
Av. when Trial 1 is excluded		.50	.53	.51	.56
Means	8.84	9.65	9.61	10.08	10.05
S. D.	1.59	1.56	1.73	1.42	1.40

Av. r is 0.463; av. when Trial 1 is excluded is 0.524.

or above 13, and that one out of five would be classified below 7 or above 15. In view of such large errors in classification of individuals, there seems to be hardly any value at all to the Knox Cube Test. This conclusion may be reënforced by the statement of Mateer that "children whose mental ages differ as much as ten years may score the same on this test."

(2) Reliability of the test with adults. In the case of 100 adults, the situation, with respect to reliability, is practically the same. The intercorrelations given in Table IV are all rather low, the average being only 0.46. If Trial 1 is omitted, the average of the intercorrelations of the remaining trials is 0.52.

Table V shows that a lengthening of 15 times would be required to attain a reliability of 0.90 if 0.37, the average of the correlations of the Trial 1 with each of the others, is used as the original reliability coeffi-

TABLE V

PREDICTED RELIABILITY OF TRIAL 1 IF THE TEST WERE LENGTHENED

(Av. correlation of Trial 1 with other trials = 0.37)

	Times lengthened					
	2	3	4	5	10	15
Predicted r	.54	.64	.70	.75	.86	.90

cient. According to Table VI, if 0.50, the average correlation of Trial 2 with each of the later trials is used, a lengthening of 9 times is still required.

We saw that with the data on the children summarized in Table I, the average intercorrelation of all trials (0.36) and the average intercorrela-

tion of the last seven trial (0.40) were both lower than the average correlation of Trial 2 with every one of the later trials (0.43). The data on adults, summarized in Table IV, show that the average correlation of Trial 2 with every one of the later trials is 0.50, the average intercorrelation of all trials is 0.46, and the average intercorrelation of the last four trials is 0.52.

TABLE VI

PREDICTED RELIABILITY OF TRIAL 2 IF THE TEST WERE LENGTHENED

(Av. correlation of Trial 2 with later trials = 0.50)

	Times lengthened				
	2	3	4	5	9
Predicted r	.67	.75	.80	.83	.90

Assuming this latter figure to be the reliability of the test, Table VII indicates what the reliability coefficients would be if the test were lengthened. Even by taking this highest figure as the reliability of a trial a lengthening of eight times would still be required in order to attain a reliability of 0.90.

As for the children, an empirical check on the values predicted by means of the Spearman-Brown formula was attempted. The correlation between the sum of the scores of Trials 2 and 3 and the sum of the scores of Trials 4 and 5 were found to be 0.71. If 0.52 is considered to be the initial reliability of the test, Table VII shows that doubling the test should yield a coefficient of 0.68. The difference between empirical and theoretical result is only 0.03. If 0.50 is used as the initial reliability, Table VI shows that by formula doubling the test should yield a reliability of 0.67. This is 0.04 smaller than the empirical result 0.71.

TABLE VII

PREDICTED RELIABILITY OF THE LAST FOUR TRIALS IF THE TEST WERE LENGTHENED

(Av. intercorrelation of last four trials = 0.52)

	Times lengthened				
	2	3	4	5	8
Predicted r	.68	.77	.81	.84	.90

For the adults the probable error of a raw score calculated from the standard deviation and reliability of Trial 1, and the probable error of a raw score calculated from the standard deviation and reliability of Trial 2 are:

Trial	P.E. $_{r.s.}$	S.D.	Reliability coefficient
1	1.00	1.59	.37
2	.91	1.56	.50

Both of the probable errors, 1.00 and 0.91 are larger than were these values for the group of children. Thus we see that although in the case of adults the intercorrelation coefficients are on the average slightly higher than in the case of the children, the probable error of raw scores is somewhat larger for the adult group.

It may clearly be seen from the foregoing that almost an innumerable number of variables, most of which are not under control, will determine the results of this test, of so-called "imitation." We may mention here, in spite of Knox's assumption to the contrary, that learning enters into this test and is taking place from first to last trial both for children and adults. The different degree of ability in the various individuals for learning such a task as this test presents, then will, of course, be a very definite factor of disturbance in our rankings.

TABLE VIII

CORRELATION BETWEEN VARIOUS TESTS AND THE KNOX CUBE TEST

Test	r	P.E.
Pressey's Arithmetic Test in Reasoning	.02	.09
Monroe Multi-Mental Scale	.14	.10
Monroe Arithmetic	.09	.10
Hudleson's Composition Test	.12	.09
Gates Silent Reading Test	.21	.09
Rorschach Imagination Test	.00	.09
Terman Intelligence Form A	.25	.09
Terman Intelligence Form B	.01	.09

(3) Quantitative results and their relation to other measures supposedly involving intelligence. In order to find out the possible influence of general intelligence or certain special abilities, the 50 children were subjected to standard tests and the results were correlated. These data are listed in Table VIII.

The abilities tested through the above tests (as claimed by the respective authors of them) such as reasoning, logical memory, ability to organize, imagination, skill in figuring, etc., apparently do not enter into the performance on the Knox Cube Test to any significant degree. The highest correlation was found to be between the Terman Intelligence, Form A, and our test, but as will be seen this also is exceedingly low (0.25).

It might be appropriate here for further illustration of the discrepancies of Knox Cube scores with measures of general intelligence to present the table above which offers an outlook over the relationship between Knox Cube scores and Terman I.Q. for 14 pupils in the Opportunity Room of the Troy Public School.

(4) Qualitative aspects of the Knox Cube Test. We first endeavored to discover the variety of methods of procedure employed in solving a task of the nature of this test. Pintner tried to throw some light on this by occasionally asking the candidates how they did it. We questioned all our *O*s at the end of each sitting: "I wonder if you can tell me exactly how you remembered to tap the cubes in the same order as I did." This generally brought full spontaneous reports from both children and adults. An amazing array of differences of procedure was apparent.

In regard to the children, we first of all made it a point to find what percentage would be stable, *i.e.* would use one and the same method throughout all consecutive trials in spite of the rather long time interval. Five out of the group of 50 children (10%) reported the same method throughout every one of the 8 sittings, never at any time varying that procedure. When these children were tested one year after the eighth trial, they again reported identically the same procedure. Of the remaining 45, the majority did not only jump from one procedure to another at consecutive sittings but would change their procedure frequently from trial to trial within one and the same sitting. All experiential reports from the *O*s were taken down verbatim, and analyzed. On the basis of these analyses it was possible to identify at least two types of procedure used by the children. These we denote as (1) voco-motor procedure, (silently naming the cubes in some way or other, A, B, C, etc., or by number, 1, 2, 3, etc.) for example: "I called each cube a number and then said the numbers as you tapped them," "I called the cubes A, B, C, D, and then said the letters to myself as you tapped that certain cube." Thirty percent of the children used this method predominantly. (2) Purely visual memory (apparently relying on pure sensory visual memory without the aid of any cues) examples are: "I don't know, I just watched," "I did just what you did," "I got a picture of what you did and did the same thing." Forty-four percent of the children used this method predominantly. A few used what we denoted as "auditory" (guided by the sounds of the taps). Examples are: "I listened for one melody which I sang to myself as you tapped the cubes, each one had a slightly different sound," "I listened for the difference in the time it took to go from one cube to the other."

The remaining children were so changeable from trial to trial and from sitting to sitting in regard to the above procedures that we found it impossible to typify them.

On the basis of all the above, and especially with later reference to the bearing of these various procedures upon the quantitative scores, we do not find ourselves able to accept the summary statement of Pintner.

Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves.[10]

The type of procedure found by Pintner in his standardization of the test, which he calls "the error of perseveration" was never found as was to be expected from Pintner's own statement that this is seldom met with among older children.[11] "The error of reverse order" occurred three times, twice on line A and once on line X.

We found with the adults, as with the children, a great variety of procedures with two main groups predominating; namely, (1) the voco-motor and (2) the purely visual memory. This is contrary to what might be expected from Pintner.

The other kind of counting which assigns constant numbers or letters to each block was found to be very rare. We found it only in about twenty cases out of our 867 individuals. This kind of counting did not seem to occur to the majority of adults who were tested by us.[12]

Twelve percent of the adults showed consistency in using one and the same procedure throughout as has been found among 10% of the children. Thirteen percent fluctuated regularly in their procedure in one and the same sitting as compared to 20% of the children.[13]

(5) Relationship between type of procedure and quantitative scores. It

[10] R. Pintner, Standardization of Knox Cube Test, *Psychol. Rev.*, 22, 1915, 377-401.

[11] *Ibid.*, 397.

[12] *Ibid.*, 380.

[13] Some said: "I organized the cubes which you tapped into a sort of pattern, so that I took one in relation to the other, all of them together forming a unit." Seventy-four verbal reports out of a total of 252 in the purely visual memory group were of this type. These had superior scores.

Another group who "just watched" discovered that the difficult combination was built upon the simple basic pattern of the scheme of presentation and so focused their attention on the new added part for learning in each successive trial of the sitting. There were some in the visual group who said they "just looked" or "just did what you did." Others gave reports as the following: "I saw the curves you made as you touched the cubes, going from one to the other. At the same time I could see the straight lines from one cube to another and I formed a mental picture of the figures made."

There were still others who said, "I 'felt' keenly the number of taps as a sort of rhythm. The rest of the procedure was visual." Those who organized in patterns on "feel" of rhythm entertained an unpleasant feeling if they were unable to execute the same number of taps as given by the *E*. They frequently put their cube down on what they knew was, at the time, the wrong cube, from some kind of a feeling of suspense which more or less compelled them to tap again until they had the right number of taps. The number of taps was felt as something of a rhythmic closed whole and never analyzed into smaller part wholes. The final closure was accompanied by a distinct feeling of satisfaction, whether rightly or wrongly executed. This seemed to us as example of temporal sequence as discussed by Köhler in his *Gestalt Psychology*, 164-165.

now seems natural to inquire into the question whether there is such a thing as an optimum method for doing the task. In a general way, we may state that there does not seem to be any such definite method. This statement

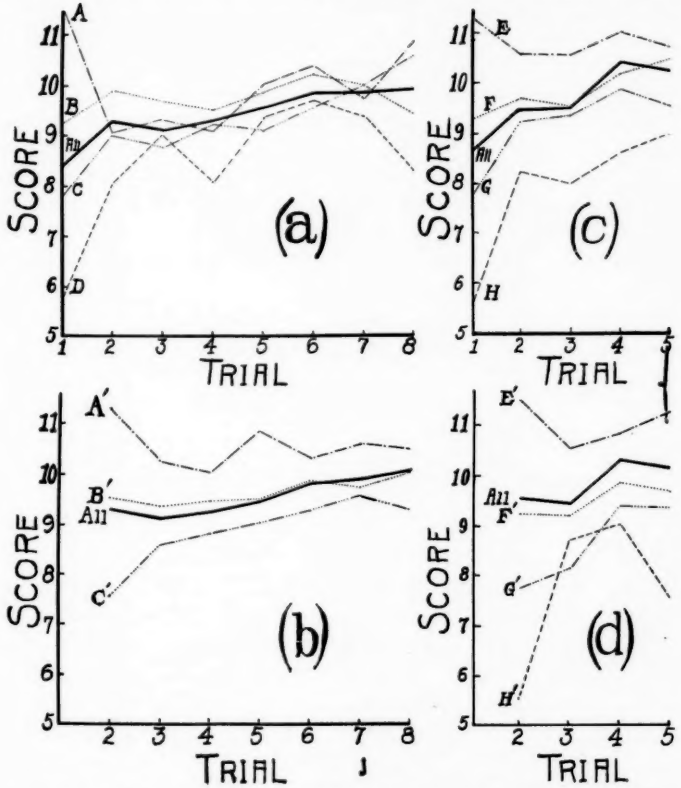


FIG. 1. CURVES SHOWING MEANS OF SUB-GROUPS FOR EVERY SUCCESSIVE TRIAL
(a) Children: Sub-groups selected on basis of score on Trial 1.
(b) Children: Sub-groups selected on basis of score on Trial 2.
(c) Adults: Sub-groups selected on basis of score on Trial 1.
(d) Adults: Sub-groups selected on basis of score on Trial 2.

should be qualified, however, to the effect that the few adults who always perceived the presentation as a unified whole or pattern or design, and who in each successive trial just added a new feature to a pattern formerly experienced, in other words, those who discovered and consciously used

the plan underlying the construction of the progressive series from the first to the twelfth trial, had superior scores. The visual group of the children gave the highest scores with the voco-motor group coming next, while the group which changed procedure very often gave the poorest score. With adults, however, the highest score out of our two main groups was found in the voco-motor, changeable group coming next and the visual group coming last. The finding that purely visual group of the children gets the highest score agrees well with an earlier study of Reymert.[14] The result for the adults that they do better with voco-motor procedure also seems a natural one. The fact that the children do worse while frequently changing method and that adults do better when changing method would find its explanation in the fact that, while the change with the children may be a haphazard affair, with the adults it most likely means a definite scheme for improving previous method.

TABLE IX

SCORES OF SUB-GROUPS OF CHILDREN ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 1 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 1	Trial 1	2	3	4	5	6	7	8	No. cases
A	11–12	11.17	9.00	9.33	9.17	10.00	10.33	9.83	10.67	6
B	9–10	9.38	9.90	9.71	9.57	9.95	10.14	10.00	10.43	21
C	7– 8	7.70	9.00	8.85	9.25	9.10	9.45	9.85	9.40	20
D	5– 6	5.67	8.00	9.00	8.00	9.33	9.67	9.33	8.33	3
All	5–12	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92	50

The possible relationships of general intelligence entering into either choice of procedure or persistence of procedure we inquired into by taking the upper quartile and the lower quartile of the Terman I.Q. and comparing these two groups in this regard. No clear differences were found.

(6) Learning. The curve of learning for the group of children is indicated in Fig. 1 (a) by the solid line which connects the means of the scores on successive trials. Most of the learning seems to take place between the first and second trials. Table IX shows that the difference between the means of the first and last trials is 1.22 (P.E. = 0.13), which is over 9 times the probable error of the difference. The difference between means of second and last trials (Table X) is 0.60 (P.E. = 0.13), which is nearly 5 times its probable error. Although the total amount of learning seems to be considerable, the learning from trial to trial is much less. Dividing 1.22 by 8, we find an average learning of 0.15 per trial. For the last 7 trials the average is 0.09.

[14] *Op. cit.*, f.n. 4.

The fact that more learning does not take place after the second trial may be due in part to the fact that the task has not sufficient range of difficulty for the group tested, since several of the group attained the highest possible score at the first sitting. Moreover, from the nature of the task, it is evident that it is a scale, that is, the difficulty of the items increases.

In order to determine whether there is any difference in learning between the poorer and the better children, the group was divided into four sub-groups on the basis of the scores made on the first trial.

In Table IX are given the means of the scores of each sub-group for successive trials. The extreme groups A and D show considerable variation, which is to be expected since they contain only a few cases.

Sub-groups B and C have curves which are very nearly parallel to the curve of the whole group so that it appears that there is not a great deal

TABLE X

SCORE OF SUB-GROUPS OF CHILDREN ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 2 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 2	Trial							No. cases
		2	3	4	5	6	7	8	
A′	11–12	11.33	10.33	10.00	10.89	10.33	10.56	10.44	9
B′	9–10	9.50	9.32	9.39	9.46	9.93	9.82	10.00	28
C′	7– 8	7.54	8.46	8.62	8.92	9.38	9.54	9.38	13
All	7–12	9.32	9.28	9.30	9.58	9.86	9.88	9.92	50

of difference in the improvement in scores, of the poorer and better individuals. The group was divided also on the basis of the scores made on Trial 2. Table X gives the means of the scores made by these sub-groups on successive trials. No scores of 5 or 6 were made on Trial 2. The sub-groups again show the same approximately parallel rise in Fig. 1 (b) as did the sub-groups A, B, C, and D, formed on the basis of scores on the first trial.

The adult group was treated in the same way with approximately the same results as can be seen from Table XI and XII.

It cannot be said that there is either convergence or divergence in ability to perform the task set by the test. Although in the case of both the children and the adults there is greater variability on the first trial than on later trials, it must be remembered that there is probably a limitation in range at the upper end of the distribution and that the task is a scale in which the steps increase in size.

Judging from the standard deviations of the first and last trials for both adults and children, there appears to be convergence in the ability tested. Since Peterson maintains that the coefficient of variability is the proper

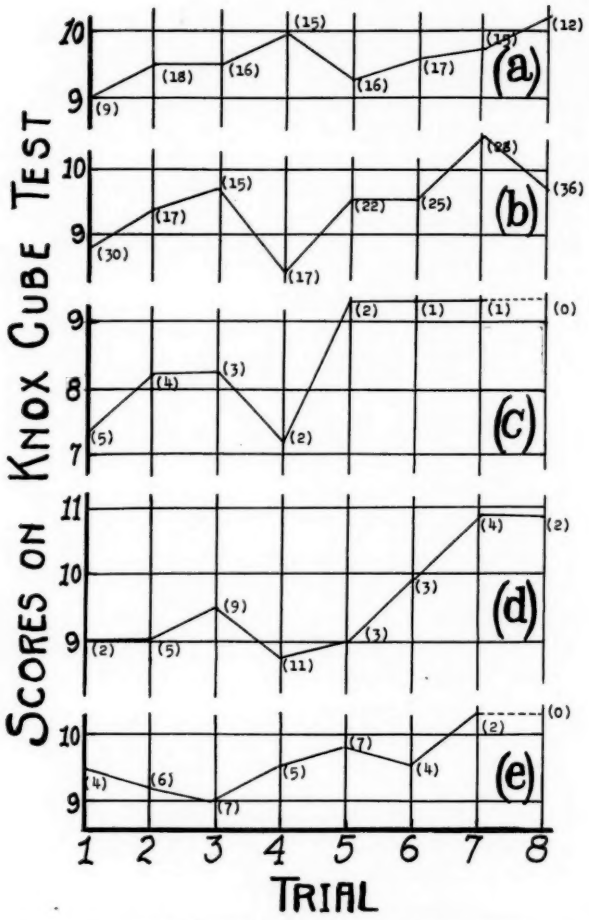


FIG. 2. LEARNING CURVES PLOTTED FROM MEAN SCORES ON SUCCESSIVE TRIALS OF CHILDREN USING VARIOUS TYPES OF PROCEDURE

(a) Voco-motor; (b) Visual; (c) Auditory; (d) Changed from voco-motor to visual; (e) Changed within trial from visual to voco-motor. Figures in parentheses give the number of subjects using the procedure.

measure to be used in determining whether there is an increase or a decrease in individual differences with practice, the coefficients of variability (S.D./M) were calculated and are presented in Table XIII.[15]

These coefficients do show a slightly greater tendency to decrease than do the standard deviations. However, that decrease of variability coefficients

TABLE XI

SCORES OF SUB-GROUPS OF ADULTS ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 1 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 1	Trial 1	2	3	4	5	No. cases
E	11–12	11.16	10.68	10.68	10.95	10.74	19
F	9–10	9.47	9.81	9.67	10.08	10.28	36
G	7– 8	7.59	9.23	9.28	9.87	9.67	39
H	5– 6	5.83	8.17	8.00	8.67	9.00	6
All	5–12	8.84	9.65	9.61	10.08	10.05	100

with practice really shows convergence in the ability tested should be questioned. Peterson did not demonstrate that. At any rate, the objections previously mentioned still apply; namely, limitation in range of difficulty of the test and inequality in difficulty of the units.

The next problem to inquire into would be the question of learning from trial to trial for the various procedures as well as for changes of procedure. Because they contain the greatest number of frequencies, or rather the greatest precent of subjects, the curves of average scores, per sitting in the visual group and that of the voco-motor group are most reliable. The curve for the voco-motor type, Fig. 2(a), for the children shows

TABLE XII

SCORES OF SUB-GROUPS OF ADULTS ON SUCCESSIVE TRIALS, WHEN SCORE ON TRIAL 2 IS CONSIDERED INITIAL ABILITY

Sub-group	Score trial 2	Trial 2	3	4	5	No. cases
E′	11–12	11.39	10.65	10.84	11.13	31
F′	9–10	9.51	9.51	9.96	9.82	49
G′	7– 8	7.75	8.13	9.25	9.25	16
H′	5– 6	5.50	8.75	9.00	7.75	4
All	5–12	9.65	9.61	10.08	10.05	100

definite, but very slightly marked learning, while the curve for the adults, Fig. 3(a), shows a great rise. This seems to be natural.

The visual group of children, Fig. 2(b), shows rather marked improvement from sitting to sitting, while we note just a slight rise for the adults in this group, Fig. 3(b). For the children a systematic change from voco-

[15] J. C. Peterson, A new device for use in teaching, testing and research in learning, *Trans. Kansas Acad. Sci.*, 33, 1930, 41-47.

motor to visual, the latter perhaps being a more natural method for children, shows a definite rise, Fig. 2(d), while the same curve for the adults has a downward trend, Fig. 3(e). On the other hand, the change from visual to voco-motor for children seems, from the curve Fig. 2 (e), just a fumbling proposition, but might have reached a conscious adult usage at

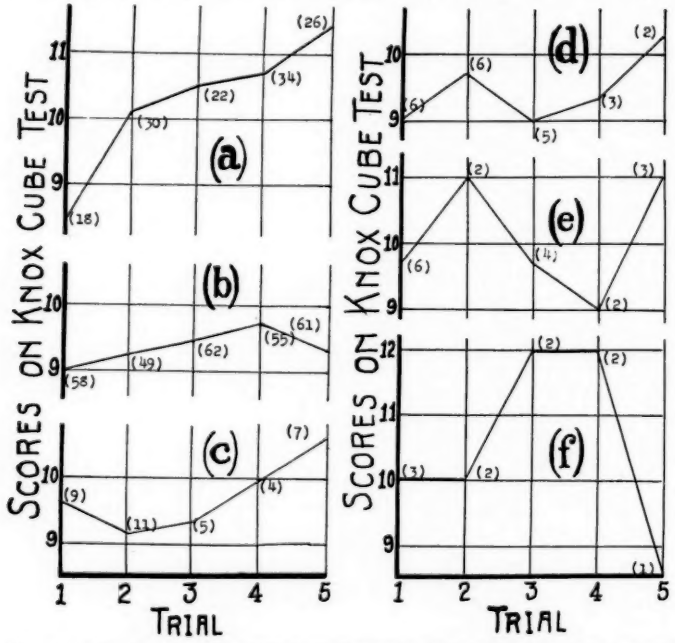


FIG. 3. LEARNING CURVES PLOTTED FROM MEAN SCORES ON SUCCESSIVE TRIALS OF ADULTS USING VARIOUS TYPES OF PROCEDURE

(a) Voco-motor; (b) Visual; (c) Visual-auditory; (d) Changed within trial from visual to voco-motor; (e) Changed within trial from voco-motor to visual; (f) Difficult patterns based upon simple ones. Figures in parentheses give the number of subjects using the procedure.

the seventh sitting, while again, as would be expected, the change from visual to voco-motor, Fig. 3(d), for adults shows more of a definite rise. From this it can be seen that the qualitative differences of method have a marked influence on the nature of the learning curve.

(7) Social status. Although this group was unselected with respect to social background, a comparison of the upper quartile with the lower quar-

TABLE XIII

MEANS, STANDARD DEVIATIONS, AND COEFFICIENTS OF VARIABILITY FOR THE VARIOUS TRIALS

		Trial							
		1	2	3	4	5	6	7	8
Adults	Mean	8.84	9.65	9.61	10.08	10.05			
	S.D.	1.59	1.56	1.73	1.42	1.40			
	S.D./M	.18	.16	.18	.14	.14			
Children	Mean	8.70	9.32	9.28	9.30	9.58	9.86	9.88	9.92
	S.D.	1.45	1.35	1.25	1.17	1.36	1.10	1.26	1.26
	S.D./M	.17	.14	.13	.13	.14	.11	.13	.13

tile as to home environment demonstrates the well-known fact that the children coming from homes of the higher social status possess, on the whole, superior intelligence (Table XIV).

TABLE XIV

COMPARISON OF THE SOCIAL STATUS OF THE UPPER AND LOWER QUARTILES

	Pupil	I.Q.*	Father	Mother	Knox Cube
Upper Quartile	A	134	Minister	College graduate	9 3/4
	B	127	Surgeon	Music teacher	11 3/8
	C	125	Doctor	Trained nurse	11 5/8
	D	123	Salesman		11 3/8
	E	120	Cook		9 3/8
	F	119	Asst. P.M.	Printer	9 7/8
	G	118	Grocer		9 5/8
	H	118	Insurance	H. S. Graduate	9 1/2
	I	117	Florist	H. S. Graduate	8 3/8
	J	115	Laborer		9
	K	114	Factory Supt.		9 7/8
	Av.				9.98
Lower Quartile	P	99	Mechanic		8 1/4
	Q	99	Butcher		8 7/8
	R	99	Factory Foreman		9 5/8
	S	98	Laborer		9 1/4
	T	98	City Fireman		8
	U	97	Laborer		9 7/8
	V	97	Factory Foreman		9 1/4
	W	94	Sheet Metal Work		9 5/8
	X	93	Odd Jobs		8 3/8
	Y	92	Factory Man		8 7/8
	Z	90	Factory Man		9 3/8
	Av.				9.03

* Computed from scores on Stanford-Binet tests.

SUMMARY AND CONCLUSIONS

The present study is an intensive exploration of the Knox Cube Test, made for the purpose of emphasizing the importance of the qualitative aspects of test results, as against the aspects gained by techniques of standardization or by refined statistical procedures.

With the method and procedure standardized by Pintner, 50 children (aged 13-14 yr.) were tested 8 times at two-week intervals, and 100 adults (college summer school students), 5 times at similar intervals. General information about the subjects, other standard test scores, and verbal reports were used as a check on the factors influencing the scores on the Knox Cube Test. Summary of the results follows:

(1) Quantitative Results. (a) As computed by the Pearson product moment method, the test from trial to trial revealed surprisingly low correlation for both children and adults. The average intercorrelation of all the trials is 0.36. Considering the first trial as practice, the average intercorrelation of those remaining is raised to 0.40.

(b) The first trial, frequently taken as the final estimate of ability, correlates least with successive trials. As calculated by means of the Spearman-Brown formula, the number of items in the first trial must be increased 25 times in order to approximate a reliability of 0.90.

(c) Taking trial two as the final score, the reliability is 0.45 and the length of the test must be increased 11 times in order to approach the reliability of 0.90.

(d) From these calculations, it may be concluded that neither theoretical nor empirical results give evidence that a satisfactory reliability might be obtained from a reasonable length of the test.

(e) The correlations of Knox Cube scores with scores of specific abilities and general intelligence tests are very low. The highest correlation, 0.25, was with the rank of I.Q. in Terman A and the rank in the first trial of the Knox Cube Test.

(2) Qualitative Results. (a) From the experimental reports, the qualitative aspects of this much used test in regard to procedures employed for solving the task, are manifold and varied, and the variations of method from sitting to sitting in the same individual seems to be the rule rather than the exception for both children and adults.

(b) Two or three typical procedures can be made out somewhat arbitrarily (voco-motor naming or numbering, purely visual sensory memory, etc.), but the relation of these to excellency of capacity is obscure. The most superior method is probably "the organization of the cubes tapped into a pattern or unit" (recalled as a whole).

(c) Comparing the types of procedure with intelligence, according to Stanford-Binet I.Q. reveals a slight relationship but is accompanied by too many exceptions to be considered significant.

(3) Learning. (a) The learning curves of the various groups reveal

marked differences between the children and adults. The children in the voco-motor group show a definite but slight rise, while the adults show a great rise. The visual group, on the other hand, shows great improvement among the children but only slight for adults.

(b) The actual learning from sitting to sitting was slight, averaging 0.15 per trial in raw score and being greatest between the first and second trials.

(c) Dividing the subjects according to scores on the first trial, it cannot be said that there is either convergence or divergence in ability throughout consecutive sittings. There is greater divergence on the first sitting but this might be attributed to the fact that there is a limitation in the range of difficulty which may account for lack of greater divergence in later sittings.

(4) Social status. Although the data indicate that the median intelligence levels of groups of varied social status differ, they do not point to a uniform grade of ability in any group.

(5) General conclusions. In spite of seemingly adverse facts against using the Knox Cube Test as a measure of ability, it may still prove useful in helping to differentiate subnormal levels of general intelligence. It is hoped that this study will suggest that, in order to use such tests as a finer measure of ability, there must be a qualitative analysis accompanying the quantitative standardization of the data.

After analysis of the scores obtained on such a simple test of intelligence, it would appear that very little information is gained concerning ability by knowing an individual score.

RELATION OF EYE-MOVEMENTS TO VISUAL IMAGERY

By CRAWFORD GOLDTHWAIT, Harvard University

The fact that the visual perception of an object is usually accompanied by movements of the eyes suggests the possibility that the centrally excited visual image of such an object possesses an efferent component of ocular movement. Experiments by G. M. Stratton,[1] and Judd, McAllister and Steele,[2] among others, have demonstrated that in visual perception the eyes do not necessarily follow accurately the outlines of the object, so that it is scarcely to be expected that they would do so in the case of an image. Nevertheless, there remains the possibility that the amount or character of the movements may bear some relation to the type of image or to one or more of its aspects.

In addition to the numerous experiments concerned with imagery as such there have been a few which have emphasized the accompanying motor activity and have attempted to discover relationships between the aspects of the visual image and characteristics of the simultaneously occurring eye-movements. One such experiment, carried out by C. W. Perky,[3] yielded results pointing to the conclusion that visual "memory images" (images of particular things, in a particular context and having personal reference) were accompanied by a markedly greater amount of eye-movement than were "images of imagination" (images of things recognized but lacking the particularity characteristic of "memory images"). Perky found also that images of the memory type were usually "scrappy" and "filmy," while those of imagination were clear and stable. Detection of eye-movement was made dependent upon the *O*'s awareness of faint luminous spots (on a dark field) normally falling within the blind spot but so placed as to stimulate sensitive marginal areas when the eye wandered from its position of fixation. Experiments by L. J. Martin[4] and R. M. Ogden[5] failed to support

* Accepted for publication October 1, 1931.

[1] G. M. Stratton, Eye-movements and the aesthetics of visual form, *Philos. Stud.*, 20, 1902, 336-359.

[2] C. H. Judd, C. N. McAllister, and W. M. Steele, Introduction to a series of studies of eye movements by means of kinetoscopic photographs, *Psychol. Monog.*, 7, 1905, (no. 29), 1-16, and following articles.

[3] C. W. Perky, An experimental study of imagination, this JOURNAL, 21, 1910, 422-452.

[4] L. J. Martin, Die Projectionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder, *Zsch. f. Psychol.* 61, 1912, 322-546.

[5] R. M. Ogden, Experimental criteria for differentiating memory and imagination in projected visual images, *Psychol. Rev.*, 20, 1913, 378-410.

Perky's findings with respect to the characteristics of memory images and images of imagination (neither *E* used apparatus for recording ocular movement), and H. Clark,[6] as a result of experiments similar to Perky's, concluded that the amount of eye-movement varied, not with the type of image involved, but with "conditions of attention." For detecting eye-movement Clark used an improved form of Perky's apparatus, the nystagmograph of A. Schackwitz and the method of directly observing through an enlarging glass a point of light reflected from the cornea.

The experiments described below constitute an attempt to resolve the inconsistencies of the earlier work. Objective records of eye-movements seemed highly desirable for a foundation and two recorders were therefore utilized, yielding, it is believed, a rather high degree of accuracy for the small movements involved. One, the *mirror-recorder,* recorded movements of the closed eye by means of a small mirror held against the lid by a device designed by R. Dodge.[7] The mirror turned on a vertical axis when the bulge of the cornea moved behind the lid, and the beam of light which it reflected moved across a sheet of flashed glass on which was focused a camera with motor-driven film. It permitted the recording of horizontal movements only, however, and was replaced by the *ciné-recorder.* This device was made up of a cine-matographic camera, taking pictures directly of the open eye, and machinery for projecting the film, frame by frame, and recording changes in the position of the iris. Both recorders were so designed that head-movement was eliminated from the final record, and both were equipped with appropriate mechanisms for recording the giving of the stimuli, the beginning and end of the image, identification of the record and time in fractions of a second.

Experiment I. The mirror-recorder was used. The *O* was instructed to get a visual image based on the substantive spoken by *E.* He was to signal when the image appeared and disappeared and was then to describe it. No reference was made to type of image in the instructions. The substantives, of which 80 were used, were chosen to render memory images available and to occasion as little eye-movement as possible.

There were 4 *Os,*[8] who gave 198 records. The images were classed as M-images (those of specific objects, in a particular experiencial setting), S-images (those of specific objects, but lacking the particular setting) and

[6] H. Clark, Visual imagery and attention, this JOURNAL, 27, 1916, 461-492.
[7] R. Dodge, A mirror recorder for photographing the compensatory movements of closed eyes, *J. Exper. Psychol.,* 4, 1921, 165-174.
[8] Eight *Os* took part in these experiments: three seniors, four graduate students and one member of the faculty.

I-images (those of no specific object). They were also classed, in accordance with the *O*'s report, as clear, moderately clear or dim; as stable, moderately stable or fluctuating.

In Table I, which gives the results of this experiment,[9] it will be seen that only in the case of dim imagery is the amount of movement sufficiently different from the general average to be of significance. The figure for "moderately stable," being the datum for only one image, is of little experimental value.

Experiment II. With the exception that the 4 *O*s were instructed to try for "images that represent objects which have had a place in your remembered experience" (M-images) in half of the cases, the procedure was the same as for Experiment I. The yield was 238 records, data for which appear in Table II.

TABLE I

SHOWING THE RESULTS OF EXPERIMENT I

	M-image	S-image	I-image
No.	17	34	147
Av. movement	31	21.2	23.6
m.v.	29.4	14.5	16.3
	Clear	Mod. Clear	Dim
No.	150	16	32
Av. movement	20.2	19.3	43.1
m.v.	12.7	13.8	33.8
	Stable	Mod. Stable	Fluct.
No.	181	1	16
Av. movement	23.4	168	19.7
m.v.	16.5	—	13.0

The tendency here for dim imagery to be accompanied by larger amounts of eye-movement is somewhat obscured in the table because 2 *O*s reported no "dim" images but registered markedly increased movement for their "moderately clear" images. Again, the significance of the data for stability of images is lessened by the facts, first, that 2 *O*s showed a definite tendency toward decreased movement for the less stable imagery, and, second, that the general trend was in opposition to that of the earlier experiment.

Experiment III. Following a short preliminary experiment with the ciné-recorder, the meager results of which were in agreement with those of the previous work, it was determined to use that apparatus in obtaining a series of images graded (by the *O*) on a clearness scale of 1 to 10, in

[9] Data in Tables I and II are in terms of arbitrary units of length as measured on the developed film. They represent amounts of eye-movement per unit of time during the presence of the images.

order that correlations might be worked out between degree of clearness and amount of accompanying eye-movement. There resulted 115 records. Coefficients obtained for the 4 *O*s were —0.60 ± .082, —0.60 ± .079, —0.56 ± .085 and —0.62 ± .075, indicating a marked tendency for images of the greatest clearness to be accompanied by the least eye-movement. The four subjective scales were not, of course, comparable, but the coefficient —0.46 ± .049, obtained from the combined data for all *O*s, reveals a fair degree of uniformity.

Experiment IV. In order to get a basis for comparison with Perky's characterizations of memory and imaginative images, 4 *O*s were asked for

TABLE II

SHOWING THE RESULTS OF EXPERIMENT II

	M-image	S-image	I-image
No.	33	76	129
Av. movement	24.8	33.4	26.9
m.v.	18.5	20.5	17.1
	Clear	Mod. Clear	Dim
No.	161	61	16
Av. movement	26.0	33.3	38.1
m.v.	16.4	12.9	19.0
	Stable	Mod. Stable	Fluct.
No.	170	45	23
Av. movement	26.1	34.6	36.2
m.v.	16.0	13.9	21.0

careful reports on 10 images of each kind, attention being directed to clearness, stability, detailedness, completeness and duration. Stimulus words were of the same sort as those used in the other experiments and the program was continued until the *O*s themselves were satisfied that they had experienced 10 representative images of the two classifications. Data of these 80 images are tabulated in Table III.

Turning first to attributes of image-type, we note that, so far as a distinguishing trend is perceptible in Experiment IV, it is that memory images are not more "filmy" or "scrappy" than images of imagination, a result which agrees with those of Martin, Ogden, and Clark. Moreover, the first three experiments, involving more than 500 images, show a scatter of unclear imagery over all classes.

When amount of eye-movement is examined, however, it is found that the unclear imagery, of whatever class, involved the most movement. When an *O* is confronted with a dim image which he is expected to describe later, it is reasonable to suppose that his attentive effort is greater than when a clear and brilliant bit of imagery presents itself to him. In such a situation

we find that his eye-movements are generally increased, a result in consonance with Clark's conclusions and tending to support Washburn's contention that "the essence of attention is movement."[10] This is not to say that all ocular movement during imagery is to be attributed to the effort of secondary attention, but only that this effort seems to be the most important factor under these experimental conditions. The rather small range within which the individual coefficients of correlation between clearness and movement fall suggests that attention (adjusted to degree of clearness) plays a definitely circumscribed part and that other factors, perhaps physiological ones, take rôles as definitely proportioned.

TABLE III

SHOWING THE RESULTS OF EXPERIMENT IV

		Memory (40 cases)	Imagination (40 cases)
Clearness	Greater	20	11
	Equal	7	
	Uncertain	2	
Stability	Greater	19	20
	Equal	1	
	Uncertain	0	
Detailedness	Greater	21	12
	Equal	1	
	Uncertain	6	
Completeness	Greater	13	8
	Equal	12	
	Uncertain	7	
Duration	Greater	19	14
	Equal	4	
	Uncertain	3	

CONCLUSIONS

(1) There is no characteristic relation between type of visual image and amount of accompanying eye-movement.

(2) Unclear visual images are usually accompanied by a greater amount of eye-movement than are clear images.

(3) The amount of eye-movement tends to increase when conditions are such as to demand greater attentive effort.

(4) Characteristics such as clearness, stability, detailedness, completeness and duration cannot be assigned with much assurance in greater degree to one type of image than to another.

---

[10] M. F. Washburn, *Movement and Mental Imagery*, 1916, 35.

THE ADAPTATION OF CUTANEOUS PAIN

By MARYLAND BURNS, Columbia University, and KARL M. DALLENBACH, Cornell University

Despite reports of the adaptation of pain published by Von Frey,[1] Murray,[2] Goldscheider,[3] and Straus and Uhlmann,[4] we are told that "the pain sense does not appear to show the phenomenon of adaptation,"[5] or, as is true of most books, the topic is altogether ignored. This may mean that the authors of these books were unconvinced by the experimental evidence, or that they were unfamiliar with it. In any case, the need for further work is apparent. Since the question is of systematic importance, and since none of the studies, with the exception of Straus and Uhlmann's, was primarily concerned with pain adaptation, the purpose of the present study is to consider whether the phenomenon does occur; and if so, what are the sufficient and necessary conditions for its appearance.

Method and procedure. An area of the skin from the middle of the dorsal surface of the left forearm was chosen as the experimental tissue.[6] This area, extending longitudinally, was 3 x 8 cm. in extent, and 10 cm. from the ulnar prominens. It was shaved, so as to avoid the distracting experiences aroused by stimulation of the hair; and, to soften the skin, it was scrubbed, at the beginning of every experimental period, with soap and water and a hand brush.

After the tissue had thus been prepared, *O* seated himself in a comfortable chair at a table, and placed his hand and forearm in a plaster-of-paris arm-rest that had been especially cast for him. His arm, held firmly in a fixed position during the periods of stimulation, was therefore relatively free from accidental movements and tremors. When *O* had placed his arm in the cast, *E* adjusted the stimu-

* Accepted for publication May 29, 1931. From the Psychological Laboratory of Columbia University.

[1] M. von Frey, Beiträge zur Physiologie der Schmerzsinnes, *Ber. ü. d. Verhandl. d. königl. sächs. Ges. d. Wiss. z. Leipzig, math.-phys. Kl.,* 46, 1894, 193; *Untersuchungen über die Sinnesfunktionen der menschlichen Haut,* 1896, 242.

[2] E. Murray, A qualitative analysis of tickling: Its relation to cutaneous and organic sensation, this JOURNAL, 19, 1908, 304 f.

[3] A. Goldscheider, Über Schmerz und Schmerzbehandlung, *Zsch. f. physik. u. diät. Therap.,* 19, 1915, 289-309; *Das Schmerzproblem,* 1920, 20, 71.

[4] H. H. Straus and F. R. Uhlmann, Adaptation of superficial pain, this JOURNAL, 30, 1919, 422-424.

[5] E. B. Titchener, *A Text-Book of Psychology,* 1909, 154.

[6] Straus and Uhlmann used the ventral surface. We believe, however, that the dorsal surface is to be preferred. For not only is it more homogeneous than the ventral, showing fewer arteries, veins, and tendons; but work upon it is less fatiguing to the *O*s, as the arm can be held more easily and steadily with dorsal side up.

lator—a modified algometer that could be applied mechanically at a constant rate and with unvarying intensity[7]—so that the stimulus-point was approximately 1 mm. above the surface of the skin. At a prearranged signal, *E* then brought the point to bear upon the skin by lowering the entire apparatus with the rack-and-pinion gear of the universal support. The stimulus was, therefore, applied at once with its full intensity. Three intensities were used: 2.5, 5.0, and 7.5 grm.[8] Every stimulus-intensity was applied in the main experiments 20 times in haphazard order to every *O*.

Three *O*s, *A*, *B*, and *C*, all graduate students and members of the class in advanced experimental psychology served in the experiments. Though none of them had previously observed in cutaneous experiments, all were familiar with the problem of pain adaptation. No attempt was, therefore, made to conceal from them the object of the present study. On the contrary, advantage was taken of their knowledge, as the following instructions, given at the beginning of every experimental period, clearly show.

> At a "ready, now" signal you will be stimulated with a sharp point. Report immediately the experiences aroused, and then describe the temporal course of your experiences. Give particular emphasis to the report of the appearance and disappearance of pain.
>
> Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the "ready, now" signal and do not open them until told to do so.

O's oral reports were recorded by *E* together with the time at which they were made, as indicated by a stop watch. *O* kept his eyes closed from the starting signal to 20 sec. after his last positive report, when the experiment was discontinued. Between experiments, *O* was permitted a short rest, during which he was free to remove his arm from the cast and to walk about the room.[9]

A short preliminary series of experiments familiarized the *O*s, as well as the *E*, the junior author, with the routine of the work and the nature of their tasks. During this and the main series, the apparatus was concealed from the *O* by a cardboard screen. In the main series, 10 experiments were performed during every experimental period, which lasted from 1½–2 hr. An experiment was not started until the after-effects of a previous experiment had worn off.

The pain spots stimulated were not localized in advance. Their stimulation was left to chance, the needle being applied haphazardly to different points of the experimental area. If pain was aroused, the experiment was allowed to run its course; if *O* reported merely pressure, the apparatus was shifted to another spot. Such in-

---

[7] A hollow brass cylinder, approximately 15 cm. long, formed the body of the apparatus. Through this, in holes which had been carefully centered, projected a thin rod (a steel knitting needle). The stimulus-point (a No. 10 sewing needle) was attached to one end of the rod, and a weight-holder to the other. When the apparatus was held perpendicularly, the rod could be moved freely up and down through the cylinder with practically no side-play.

[8] The values, obtained by means of a pressure-balance, include the slight friction of the instrument, and the weight of the stimulus-point, connecting rod, weight-holder, and weights.

[9] *O* was not required to use a blindfold or to retain his arm in the rest between experiments because we wanted him to be keenly alive, for the duration of the experiments, to his experiences, and not become drowsy and sleepy—conditions which would of themselves make for adaptation.

stances, however, were not numerous, because of the care taken in preparing the tissue and of the large number of pain spots in the experimental area.

Results. (1) Adaptation. We obtained reports of the adaptation of pain in every one of the 60 experiments with every *O*. The time of adaptation, however, varied considerably: from 6–780 sec. for *A*, from 4–160 sec. for *B*, and from 5–110 sec. for *C*. The average adaptation-times, given in Table I, also show large individual differences and wide fluctuations. The m.v. of the averages vary, for example, for *A* from 43–71%, for *B* from 55–69%, and for *C* from 46–59%. These variations are so large and the

TABLE I

SHOWING, FOR EVERY *O* AND FOR EVERY WEIGHT OF STIMULUS, THE AVERAGE TIME OF ADAPTATION IN SECONDS, THE M.V. FROM THE AVERAGE, AND THE RANGE OF THE DISPERSION

(60 experiments for every *O*)

O	Stimulus weight in grm.	Adaptation time in sec.			m.v./av.
		Av.	m.v.	Range	
A	2.5	43.1	18.6	12– 75	43%
	5.0	99.1	70.1	6–302	71%
	7.5	176.9	111.2	35–780	69%
B	2.5	18.9	13.6	4– 60	69%
	5.0	36.5	22.5	5–105	62%
	7.5	42.1	23.0	6–160	55%
C	2.5	41.2	21.3	12–105	52%
	5.0	32.8	19.4	5– 90	59%
	7.5	40.7	18.9	10–110	46%
All	2.5	34.7	19.8	4–105	57%
	5.0	56.1	41.7	5–302	74%
	7.5	86.6	70.5	6–780	81%

ranges of dispersion are so great that little, if any, significance may be attached to the fact that *A*'s and *B*'s averages increase with intensity of stimulus. Though the results of these two *O*s vary in the same direction they differ greatly in amount: *A*'s averages vary from 43.1 sec. for the weakest intensity to 176.9 sec. for the strongest; *B*'s from 18.9 to 42.1 sec. *C*'s average adaptation-time, moreover, varies in the opposite direction. It is the shortest (32.8 sec.) for intermediate intensities, and largest (41.2 sec.) for weakest intensities.

Our results give no indication, therefore, that the phenomenon of cutaneous pain adaptation is conditioned, to any great extent at least, by the intensity of the stimulus. We have no explanation to offer for the fluctuations obtained. The adaptation-time seems to be entirely a matter of the

spot stimulated. It cannot be correlated with fatique or ennui, for the fluctuations were as marked during the first as during the last part of an experimental period, and during the early periods as during the later.

Straus and Uhlmann suggest that their long adaptation-times may be due to the fact that "for all our precaution, some slight movement of the observer's arm may have taken place,"[10] the inference being that if movement had taken place the pain experience would be prolonged or revived. They are led to this suggestion by their belief that movement inhibits adaptation. They state that "it is essential to success [*i.e.* adaptation] that there be no movement either of the stimulus-point or of the skin beneath the stimulus, and also that the intensity of the stimulus remain constant during the period of stimulation."[11] In order to test this suggestion we varied our procedure slightly during the last two sittings (*i.e.* 20 experiments) with every *O*. Five seconds after *O* reported complete adaptation, we instructed him to wriggle his hand and fingers, or to move his forearm slightly, or to make both movements. After all our precautions to avoid movement we were greatly surprised to discover how stable adaptation was once it had been reached, and how readily it could be obtained under conditions of movement. In 56 of these 60 experiments, *i.e.* in about 93%, pain did not return; and in the 4 instances in which it did reappear it lasted only for a brief interval and adapted again in spite of the fact that the movements were continued. Straus and Uhlmann's tentative explanation of the long adaptation-time is, therefore, untenable; and so also is their contention that motionlessness is the *sina qua non* of pain adaptation. We are, in the light of these results, led to the conclusion that movement in and of itself does not inhibit adaptation.[12] All that is required for the adaptation of cutaneous pain is a stimulus of unvarying intensity. Movement is effective, we believe, only when and insofar as it makes for an increase of intensity.

(2) The temporal course. The temporal course of the phenomenon of pain adaptation varied among the different *O*s, and at different times and spots for the same *O*. The course of adapation appears to be conditioned by the spot stimulated, and not by any of the objective conditions, such as stimulus-intensity, over which we had control. Sometimes pain appeared as soon as the needle-point was brought into contact with the skin, at other

---

[10] *Op. cit.*, 424. [11] *Ibid.*, 423.

[12] Once this has been pointed out, it seems obvious; for instances are frequent in every day life. Hot water, for example, which is at first painful to the hands, becomes indifferent with continued use. The pain does not reappear when the hands are moved.

times there was a short delay. The average time of report was 2.2 sec. for *A*, 1.5 sec. for *B*, and 1.9 sec. for *C*. The *O*s were, however, under no constraint to hasten their reports. They frequently delayed their reports in order to make certain of them; consequently the values just given cannot be regarded as indicative of the time of the appearance of the pain. These values merely mark the first step in our *O*s reports of the temporal course of their experiences.

(a) Normal type. All of our *O*s, in all of the experiments, reported the incursion of pressure experiences in the course of adaptation. The normal or most frequent course of adaptation was a gradual decline in the intensity of the pain, which was followed by the disappearance of pain and the emergence of pressure, and the decline and disappearance of pressure. The following is a composite of the reports of these typical cases: "(1) sharp intense pain, (2) sharp pain, (3) pain, (4) dull pain, (5) weak pain, (6) pressure, (7) weak pressure, (8) tickle, (9) nothing." At which point did the adaptation of pain occur? Between (5) and (6) or at (9)? Our answer will depend upon our point of view. If we fall into the stimulus-error and ask at which point is the pain *stimulus* adapted, our answer will obviously be at (9); but if we adhere to the qualitative point of view our answer will just as patently be between (5) and (6). The quality of pain is adapted when the quality disappears. As stated above, however, we continued our experiments until 20 sec. after *O*'s last positive report of an experience derived from our stimulus—*i.e.* 20 sec. after (9) had been reported—because we wished to bring our work into line with the earlier studies.

Von Frey[13] and Goldscheider[14] were both concerned with the temporal course of the phenomenon and they arrived at different conclusions. Von Frey found that cutaneous pain, under continued and weak stimulation, became weaker and weaker and finally disappeared. Goldscheider found, on the contrary, that pain passed over into a new "unterschmerzlichen Empfindung" before complete adaptation was reached. The difference between these results is more apparent than real. The answers depend upon the form of question asked. Our results corroborate Goldscheider, but they were obtained in answer to the question: What is the course of experience in the adaptation of painful stimuli? If our question had been: What is the course of experience in the adaptation of pain? we might have obtained results confirming Von Frey. Our experimental conditions were not set to answer that question. Our stimuli were applied at superliminal pressure values. They aroused pressure as well as pain experiences. That the pressures reported by our *O*s may have been nothing more or less than the residual effects of our own stimuli, is borne out by unchecked and uncontrolled observations. The adapta-

[13] *Op. cit.*

[14] *Op. cit.*

tion of the hands, for instance, to the pain of hot water does not seem to pass through a stage of pressure. The only way, however, of deciding this question is to arouse pain with stimuli, for example radiant heat, that do not give rise simultaneously to pressure experiences.

(b) Variant types. In addition to the normal course of adaptation, which starts with maximal pain and gradually subsides through pressure to indifference, two other types were reported. In one type, as the following report shows, maximal pain recurred once:

A. 2.5 grm.: Sharp pain (3″);[15] dull pain (8″); pressure (12″); sharp pain (15″); dull pain (25″); pressure (37″); adaptation (40″).

In the other, pain-maxima recurred twice:

B. 7.5 grm.; Sharp pain (1″); dull pain (13″); very sharp pain (16″); pain (25″); very sharp pain (35″); dull pain (40″); pressure (45″); nothing (50″).

These types were reported by all the *O*s, and as frequently in one part of the experiment as in another. We thought, when they first occurred, that they might be due to accidental movements of the stimulus-point, or of the tissue under the stimulus-point; but when we tested this possibility we found no confirmation of it:[16] pain recurred when there was no movement; and it did not invariably or even frequently recur when there was movement. Of the other possible explanations considered—changes in breathing and pulse showed nothing—volumetric change seems the most hopeful. We did not, however, have an opportunity of testing it. Indeed, we do not know, as a matter of fact, whether volumetric changes occurred during our experiments, but from the results of other studies it seems probable that they did occur.[17] If they did, the changes from a smaller to a larger volume may have, by an upward thrust against the stimulus-point, increased momentarily the intensity of the stimulus and thus the intensity of the experience. This explanation of the recurrence of pain-maxima is theoretically plausible; the test by experiment remains to be done.

(3) After-sensations. The removal of the stimulus was frequently followed by after-sensations which were so insistent that the *O*s reacted to them by rubbing the area and by volunteering descriptions. The qualities most frequently reported were itch and prick, but heat, warmth, cold, pain, and pressure were also mentioned. These after-sensations endured at times

[15] This and the following values show the time in seconds that elapsed between the application of the stimulus and the report here recorded.

[16] The device used in recording these movements was constructed as follows: a small thread was fastened to the stimulus-point and the short arm of a writing-point. If the stimulus-point was moved, either by jarring or by a movement of the *O*'s arm, a record was made by the writing-point upon a kymograph.

[17] K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 408, 410.

for several minutes. The pain, including itch and prick, was usually patterned, radiating from the point stimulated up and down the radial side of the arm, and was usually localized nearer to the elbow than to the wrist. The intensity and insistence of the after-sensations seemed to be correlated with the intensity of stimulation: adaptation to the heaviest stimulus, *i.e.* to 7:5 grm., was most frequently followed by after-experiences.[18]

Summary and Conclusions

(1) Cutaneous pain, aroused by a No. 10 sewing needle at a pressure varying from 2.5–7.5 grm., is adaptable. The adaptation-times vary considerably, however, with the *O* and with the spot stimulated.

(2) Adaptation-time is not conditioned by intensity of stimulus, by fatigue or ennui, or by movement.

(3) Slight movements of the stimulus or of the tissues beneath the stimulus do not interfere with the phenomenon of adaptation. All that appears to be required in order to obtain the phenomenon is a stimulus of unvarying intensity. Movement seems to inhibit adaptation only insofar as it increases the intensity of the stimulus and makes for variability.

(4) The pain experiences aroused by punctiform stimuli are replaced by pressury experiences before the stimulus is completely adapted. Whether this is inherent in the adaptation of pain or is merely the residual effect of the pressure-pain stimulus used in this experiment is a problem for further work, that may be definitely answered by using painful stimuli which do not simultaneously arouse pressure.

(5) The normal course of adaptation, under the conditions of this experiment, is a gradual subsidence from a maximal pain through pressure to indifference. In addition to this type, two variants were obtained, which are characterized by the recurrence of intense pain. In one the maximum pain recurs once, in the other it recurs twice.

(6) The cause of the recurrence of the pain-maxima in the variant types of adaptation is not known. It does not appear, however, to be due to accidental movements of the stimulus-point or of the tissues beneath the point, nor to changes in breathing or pulse. Volumetric changes offer a plausible explanation, but one that requires the test of experiment.

[18] Since the acceptance of this paper, E. F. Wells and L. B. Hoisington (Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.*, 5, 1931, 352-367) have published a phenomenological study of pain adaptation. Their results and ours stand in close agreement. In addition, however, to the normal and variant types of adaptation described above, they obtained a non-pressury type in which "pain simply gets weaker, less dense, and fades out of experience" (361).

THE OCCUPATIONAL BACKGROUND OF FEEBLE-MINDEDNESS

By DONALD G. PATERSON and EDWARD A. RUNDQUIST, University of Minnesota

Taussig's suggestion of an intellectual hierarchy of occupations has been verified by the use of objective mental tests during the World War and subsequently by many investigators.[1] This knowledge now permits definite description of the social-economic composition of groups of children (through classification of parental occupations) or adults selected for any particular psychological study. In the absence of such methods some writers have misinterpreted the intellectual significance of the occupational information contained in their studies. A notable example is Pearl's analysis of the occupational background of eminent men.[2]

Terman[3] and Cox[4] using such occupational classification methods in their studies of genius have shown that approximately four-fifths of the fathers of gifted children and a similar proportion of the fathers of eminent men have been furnished by the upper social classes in society. These data confirm in a more systematic manner the earlier work of Galton, Ellis, Cattell, DeCandolle, Odin, Clarke, and others.

In view of the significance of the findings resulting from the use of these methods in studies of genius it is surprising to find little or no data con-

* Accepted for publication October 1, 1932. The writers gratefully acknowledge their indebtedness for aid in completing this study to the University of Minnesota Graduate School for a research grant to cover the cost of recording the data, and to Dr. J. M. Murdoch, Superintendent of the Minnesota School for Feeble-Minded, Faribault, Minnesota, for permitting and for supervising the collection of data from the official records.

[1] F. W. Taussig, *Principles of Economics. II,* 1912, 134-138; Psychological examining in the United States Army, *Memoirs Nat. Acad. Sci.,* 1921, XV, Pt. III, Ch. 15, Intelligence Ratings of Occupational Groups, 819-837; D. Fryer, Occupational intelligence standards, *School & Soc.,* 16; 1922, 273; M. E. Haggerty and H. B. Nash, Mental capacity of children and parental occupation, *J. Educ. Psychol.,* 15, 1924, 559-572; W. D. Scott and R. C. Clothier, *Personnel Management,* 1923, 643; F. L. Goodenough, The relation of the intelligence of pre-school children to the occupation of their fathers, this JOURNAL, 40, 1928, 284-294.

[2] R. Pearl, The biology of superiority, *Am. Mercury,* Nov. 1927, 257-266; The present status of eugenics, *Sociological Press,* 1928, 1-20. For reply, see Donald G. Paterson and Edmund G. Williamson, "Raymond Pearl on the doctrine of 'Like Produces Like,'" *Am. Nat.,* 63, 1929, (May-June), 265-273.

[3] L. M. Terman, *Genetic Studies of Genius,* I, 1925, 64.

[4] C. Cox, *Genetic Studies of Genius, II,* 1926, 37.

cerning the social-economic composition of the feeble-minded groups in society. One familiar with the data showing a positive correlation between social-economic status and intelligence for random samplings of the population could infer that the facts in regard to the feeble-minded would be the antithesis of those found to be true for the mentally gifted. The present study was designed to accumulate and classify objective data whereby the validity of such an inference might be thoroughly tested.

Not only will information regarding the occupational classification of the parents of the feeble-minded add to our knowledge of the mentally deficient but also such data will permit proper account to be taken of social status whenever the feeble-minded are compared with samplings from the general population in regard to head-size, height, weight, dentition, or other physical traits. One of the writers[5] experienced great difficulty in evaluating the significance of such comparative studies because of the absence of definite information concerning the social status of the feeble-minded. In the publication referred to, considerable attention was devoted to the complications introduced into the consideration of the problem of the relation between physical traits and intelligence by the factor of social status.

Source of data. The present study consists of an analysis of the occupational status of the parents of 823 admissions to the Minnesota School for Feeble-minded at Faribault. The study also involves a similar analysis for 516 applicants for admission ("waiting list") to the same institution.

Data for each case were taken from the official records and included the following items: sex, chronological age, mental age, I.Q., test employed, occupation of father, and weekly or monthly income of father. The records are gratifyingly complete due to the services of a full-time social worker who carefully investigates the family background of each case and also due to the practice of having the cases examined by qualified mental testers under the direction of Dr. Fred Kuhlmann.

The data were collected during the Spring of 1931 and may be regarded as representative of institutional experience in receiving inmates during the preceding five years. The depression had not been operative a sufficient length of time in this area to inject selective factors which would distort the institutional picture one might have gained at any time during the preceding decade.

In utilizing the mental test data it was decided to rely upon I.Q. as the most significant single index of mentality. However, in 229 cases no computed I.Q. was available because the subjects were so low mentally as to preclude definite I.Q. determination. In all such cases the notation on the records is "Idiot" and the writers assumed an I.Q. between 10 and 20. The writers also assumed the equivalence of ratings obtained from the Stanford-Binet (317 cases) and from the Kuhlmann-Binet (793 cases) for the low levels involved in this study.

---

[5] D. G. Paterson, *Physique and Intellect,* 1930, 1-304.

Treatment of the occupational data. The Minnesota Rating Scale for Occupational Intelligence was employed in classifying the parental occupations.[6] This scale is a revision of the Barr-Taussig Scale.[7]

The Minnesota Scale differs from the Barr-Taussig Scale in the following particulars. Twenty industrial psychologists furnished estimates of the intellectual level typifying each occupation. These psychologists were guided by a standard classification of occupations into six major categories. The Minnesota Scale also listed many more specific occupations (243 as against 120). Furthermore, subdivisions of occupations requiring different levels of intellect are listed as separate occupations. For example, 20 retail dealers are specified according to products sold and are actually rated by the judges in four of the six major categories.

The standard classification of occupations into six major categories was derived from a careful study not only of Taussig's classification and Barr's extension of the Taussig Scale but also of the objective mental test results as determined by the Army Psychologists and as subsequently revised by Fryer.[8] The six major categories are defined as follows.

(I) High professional and major executives:
Requiring very superior abstract intelligence with training equivalent to a college graduate from a first class institution.
High standards, with ability for creative and directive work; such as, lawyer, college president, president of a large manufacturing concern, etc.

(II) Lower professional and business men:
Requiring superior abstract intelligence with training equilavent to 2 or 3 years of college or executive of moderately large business. Achievements less creative than in Group I, but also demanding executive and leadership ability.
Executive of a moderately large business, veterinary doctor, high school teacher, etc.

(III) Technical, clerical, supervisory occupations:
Requiring high average abstract intelligence with training equivalent to high school graduation.
Minor executives (foreman, department head) or highly technical work often involving dealing with abstract classifications and details; such as, R. R. clerks, some retail dealers, photographers, telegraphers, shop foremen, stenographers, etc.

(IV) Skilled tradesmen and lower grade clerical workers:
Requiring average abstract intelligence with equivalent of some training beyond the eighth grade.
Mechanical work demanding specialized skill and knowledge; tasks mostly of a complicated but concrete nature and requiring particular technical training; such as, auto mechanic, stationary engineer, file clerks, typists, etc.

(V) Semi-skilled occupations:
Requiring low average or slightly below average abstract intelligence, with training equivalent to seventh or eighth grade.
Work demanding a minimum of technical knowledge or skill but a maximum of special abilities, such as dexterity in the performance of repetitive and routine work; such as, delivery men, packer in factories (operate machines but do not understand principles and are unable to repair or set up the machine), and lowest grades of clerical work also, such as number sorters, etc.

(VI) Unskilled occupations:
Requiring inferior abstract intelligence only, with no formal training necessary.

---

[6] E. S. Brussell, A Revision of the Barr-Taussig Scale of Occupational Intelligence, M. A. Thesis, University of Minnesota Library, 1930.
[7] See Terman, *op. cit.*, 66-72.
[8] *Op cit., School & Soc.*, 16, 1922, 273.

Routine manual work under supervision and requiring no skill or technical knowledge; such as, day laborers, R.R. section hands, etc.

In general, there was close agreement among the twenty industrial psychologists in assigning each occupation to one of the six major categories. The Minnesota ratings also agree closely with ratings of common occupations in the Barr-Taussig Scale ($r = +0.95$). Furthermore, in using the Minnesota scale for rating occupations, Brussell found that two independent ratings of a list of occupations yielded a scoring reliability coefficient of $+0.98$. Likewise, the writers independently rated the occupations involved in the present study and confirmed this result.

Results. Table I presents the main comparison of the occupational background of the two groups of the feeble-minded and of a random sample of adult males in Minneapolis.[9] It will be noted that only 12.6% of the

TABLE I

CLASSIFICATION OF OCCUPATIONS OF FATHERS OF THE FEEBLE-MINDED AND OF A RANDOM SAMPLE OF MALE ADULTS IN MINNEAPOLIS, MINNESOTA

Occupational Category	Occ'ns of Admissions		Occ'ns of Applications		Occ'ns of Random Sample of Minneapolis	
	No.	%	No.	%	No.	%
(I) High Prof. and Major Executives	3	0.4	3	0.6	26	3.0
(II) Lower Prof. and Bus. Men	34	4.1	6	1.2	69	8.0
(III) Tech., Cler., and Minor Executives	67	8.1	39	7.5	260	30.0
(IV) Skilled trades and Lower Clerical	259	31.5	170	33.0	234	27.0
(V) Semi-skilled Occupations	140	17.0	106	20.5	234	27.0
(VI) Unskilled Occupations	320	38.9	192	37.2	43	5.0
Total	823	100.0	516	100.0	866	100.0

fathers of 'admissions,' and only 9.3% of the fathers of 'applications' are classified as engaged in the upper three occupational levels, whereas 41.0% of the random sample come from these levels. On the other hand, 87.4% of the fathers of 'admissions,' and 90.7% of the fathers of 'applications' are drawn from the lower three occupational levels, whereas only 59.0% of the random sample come from these levels. In brief, it is clear that the fathers of feeble-minded persons committed to a state school come predominately from the skilled, semi-skilled, and unskilled labor groups.

If one's attention be directed to the two lowest classes (semi-skilled and unskilled) it is apparent that over half (55.9% and 57.7%) of the fathers

[9] The random sample of adult males in Minneapolis was obtained by listing and rating the occupations of 866 adult males as given in the Minneapolis City Directory, 1930. In all cases where the occupational information was ambiguous or inadequate the employer was visited and more specific information was secured. The writers are indebted to Miss Brussell (*op. cit.*) for these data.

of these institutional cases are semi-skilled or unskilled whereas only one-third (32.0%) of the random sample are similarly lacking in occupational efficiency.

To check the possibility that the above comparisons are vitiated by the inclusion of farm owners, farm tenants, and farm laborers among the fathers of the institutional cases whereas such rural occupations would be lacking in the urban random sample, the writers tabulated the above rural occupations separately. When this was done no significant change in the percentage was apparent. The small percentage of the random sample in Class VI (unskilled) is to be explained by the fact that Minneapolis is a commercial and small industry area.

Table II shows the relationship between I.Q. groupings of the feeble-minded inmates (admissions) and the classification of the occupations of

TABLE II

RELATION BETWEEN I.Q. OF FEEBLE-MINDED INMATES (ADMISSIONS) AND OCCUPATIONAL CLASSIFICATION OF THEIR FATHERS

Occupational Category	I.Q. Groups								Total	
	1–20		21–40		41–60		61+			
	No.	%	No.	%	No.	%	No.	%	No.	%
I	0	0.0	2	1.4	1	0.3	0	0.0	3	0.4
II	11	5.5	11	7.6	8	2.9	4	2.0	34	4.1
III	21	10.4	19	13.1	18	6.5	9	4.5	67	8.1
IV	96	47.5	52	35.9	75	27.0	36	18.2	259	31.5
V	39	19.3	31	21.3	50	18.0	20	10.1	140	17.0
VI	35	17.3	30	20.7	126	45.3	129	65.2	320	38.9
Total	202	100.0	145	100.0	278	100.0	198	100.0	823	100.0

their fathers into the six occupational intelligence levels. Mere inspection of the table shows the presence of a negative relationship between occupational class and I.Q. Over 75% of the admissions with I.Q. of 61 or higher come from fathers who are semi-skilled or unskilled. To a lesser extent the same trend is true for those who have I.Q. between 41 and 60. At the other end of the I.Q.-scale the reverse is found. The 'idiots' (I.Q. from 1 to 20) and the 'imbeciles' (I.Q. 21 to 40) come from fathers who are more nearly like a random sample of the adult male population. As a matter of fact, only one-third (36.6%) of the fathers of 'idiots' and only two-fifths (42.0%) of the fathers of 'imbeciles' belong to the semi-skilled and unskilled laboring classes. When the data in Table II were tabulated separately for urban and rural occupations the trend was in no way modified. The writers believe that the tendency for fathers of 'idiots' to be more representative of the general population in terms of occupational efficiency would

be more clearly evident were certain selective factors not operative. We refer to the fact that the small number of private custodial homes for the feeble-minded generally handle low grade cases whose parents are wealthy enough to afford such care. The result is that parents from Class I are conspicuous by their absence from such institutional data as is presented here.

Table III presents the results for applicants for admission. As in Table II, the trend toward an inverse relationship between I.Q. and occupational level of father is unmistakable.

The negative relationship between I.Q. and occupational level disclosed in Tables II and III is due primarily to the fact that low grade feeble-mindedness is caused, for the most part, by accidental, pathological, non-

TABLE III

RELATION BETWEEN I.Q. OF FEEBLE-MINDED APPLICANTS AND OCCUPATIONAL CLASSIFICATION OF THEIR FATHERS

Occupational Category	I.Q. Groups 1–20		21–40		41–60		61+		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%
I.....	2	1.3	1	1.0	0	0.0	0	0.0	3	0.6
II.....	2	1.3	3	2.9	1	0.6	0	0.0	6	1.2
III.....	15	9.6	8	7.8	11	6.5	5	5.8	39	7.5
IV.....	62	39.7	42	40.8	46	27.0	20	23.0	170	33.0
V.....	33	21.2	24	23.3	28	16.5	21	24.1	106	20.5
VI.....	42	26.9	25	24.2	84	49.4	41	47.1	192	37.2
Total.....	156	100.0	103	100.0	170	100.0	87	100.0	516	100.0

hereditary factors which would be distributed more or less at random among the various classes of society whereas simple feeble-mindedness is transmitted by biological heredity. In the latter group of cases it is to be expected that they (high grade imbeciles and morons with I.Q. over 30 for the most part) would spring from mentally deficient or mentally dull parents who are unable to compete economically except in the semi-skilled and unskilled levels of work. Striking evidence with respect to the non-hereditary character of idiocy in contrast to the rôle of heredity in the higher grades of feeble-mindedness is contained in the significant work of Ordahl.[10]

SUMMARY

(1) Absence of quantitative data regarding the social-economic status of inmates of state institutions for the feeble-minded led to the present study.

[10] George Ordahl, Heredity in feeble-mindedness, *Training School Bulletin*, March-April, 1919, 2-16. See also H. H. Goddard, *Feeble-mindedness: Its Causes and Consequences*, 1914, 1-599.

(2) The following items of information were secured for each of 823 admissions and 516 applicants for admission to the Minnesota School for Feeble-minded: sex, chronological age, mental age, I.Q., test employed, occupation of father, and weekly or monthly income of father.

(3) The Minnesota Revision of the Barr-Taussig Scale of Occupational Intelligence was used in classifying the occupations of the fathers of the feeble-minded admissions and applicants and in classifying the occupations of 866 adult males of Minneapolis used as a random sample for comparative purposes.

(4) The fathers of feeble-minded persons committed to a state school come almost solely from the skilled, semi-skilled, and unskilled labor groups. The three upper occupational classes contribute a relatively small number of institutional cases in comparison to their numerical strength in the general population.

(5) There is a negative relationship between I.Q. and occupational level among the feeble-minded. The occupations of fathers of 'idiots' tend to be representative of the general occupational distribution found in society whereas the occupations of fathers of the higher grades of the feeble-minded are largely confined to the semi-skilled and unskilled levels. These results suggest that pathological and accidental cases of feeble-mindedness occur on all levels of society whereas cases of simple feeble-mindedness due to biological heredity arise from parents whose low mentality confines them to the semi-skilled and unskilled levels of work.

THE CONSTANCY OF CERTAIN BEHAVIOR PATTERNS IN YOUNG CHILDREN

By ARTHUR T. JERSILD, Columbia University

This study deals with the predictive value of measurements of the conduct of young children. The subjects were kindergarten children who previously had been studied while enrolled in a nursery school.[1] A technique developed by Arrington for recording children's overt behavior, use of materials, talking, laughing, and contacts with other children was used.[2]

This technique involves simultaneous observation by two persons of the child's activity during his free play. Mimeographed record blanks and stop watches are used by the observers. One person records four types of behavior: use of material, inactive contact with material, physical activity not involving material, and no overt bodily activity. This behavior is recorded in terms of its duration in seconds. The other observer records talking, physical contacts with other children (initiated and received), laughing and crying in terms of their occurrence within 5-sec. intervals. Complete details as to the construction of the record blank and the method of recording are given in Arrington's monograph. Each observation lasts 5 min.; repeated 5-min. samplings of behavior, extending over a long period of time, are taken in the case of each child. The final score of each child is based upon twenty-four 5-min. observations.

The reliability of this technique as a whole, the reliability of the observers, and of the recording of behavior in terms of each of the various categories, the consistency of the data as compared with data in other contemporary studies, are thoroughly discussed by Arrington. From every approach, high reliability was shown.

In the present study this technique was applied once more to children who had been subjects in Arrington's investigation. The records were taken by Monema Greenough and Lena Cowles, both of whom took part in the original studies in which the reliability of the method and of the observers was determined.

As in the original study, twenty-four 5-min. records were taken of the behavior of each child. The observations were made one or two forenoons each week over a period of several months. The *Ss* included 11 of the children who had been

* Accepted for publication July 20, 1932. This study was done with the assistance of Monema Greenough and Lena Cowles.

[1] This study was conducted under the auspices of the Child Development Institute, Teachers College, Columbia University. The author is grateful to Dr. Lois Hayden Meek, Director of the Institute, for the facilities for conducting the study; for access to the kindergarten children he is grateful to Dr. Rollo G. Reynolds, Director, and Miss Alice Thorn, of the Horace Mann School, and to Dr. Jesse H. Newlon, Director, and Miss Virginia Carroll of the Lincoln School.

[2] Ruth E. Arrington, Interrelations in the behavior of young children, *Child Develop. Monog.*, 1932, (no. 8).

studied during the previous year. Four of these were enrolled at the kindergarten of the Lincoln School, 7 were enrolled in Horace Mann (both of these schools are associated with Teachers College). On a given forenoon, each *S* was observed 5 min. in turn and, if time would permit, a second series of 5-min. observations was made. The observations were rotated to provide, as far as possible, similar conditions as to time of day and day of week for each *S* in the study as a whole. The average age of the *S*s (on February 1, approximately the mid-point of the study) was 52.04 mo.; the range was from 46.7 to 60.7 mo.

As a supplementary project, observations were made of the *S*s independently of the formal observational technique, and records of case studies were obtained.

It can be seen that the environment in which the present records were taken differed from that of the original study in the nursery school. In the present study, as in the earlier one, the procedure was to take records only during the *S*'s free activities. But the kindergarten routine entails more direction usually than the procedure in a nursery school, even when no formal project is under way. Further, the *S*s when first studied were united in one group whereas in this investigation they were divided between two different groups. Every *S*, in other words, was observed in a new environment containing only a part of his previous group.

For the purpose of this study the difference between the previous and the present environment of the *S* is all to the good. The motive in studies of young children is not so much to uncover episodic features of their behavior as to discover underlying behavior tendencies. It is necessary, however, that the new environment should allow equal freedom to the two groups of children if the records of the two groups are to be combined. The procedure in the two kindergartens varied in one respect. In one school the children spent all of their time, whether at play or at work, indoors. In the other school, attended by 4 of the *S*s, it was necessary to take half of the records while the children were spending their time on the roof of the building. Steps which were taken to provide for this discrepancy are described in the treatment of the results.

Results. At the end of the study the data for each child consisted of twenty-four complete 5 min. records. These data were compiled to yield for each *S* a score under each of the following headings:

Average number of seconds per five-minute observation period spent in: (1) Use of material. (2) Contact with material (inactive). (3) Contact with material—total of (1) and (2). (4) Physical activity without material. (5) Physical activity—total of (1) and (4). (6) No overt activity (without contact with material). (7) No overt activity—total of (2) and (6).

Average number of 5-sec. intervals per 5-min. period during which there occurred: (8) Talking to persons. (9) Talking to self. (10) Talking—total of (8) and (9). (11) Physical contacts initiated by the *S*. (12) Physical contacts received by the *S*. (13) Laughing—crying, originally included in this group, very seldom occurred in the follow-up observations and was therefore excluded.

In addition to this compilation of the data for the group as a whole, the scores of the 7 *S*s who attended one of the kindergartens, and of the 4 in the other school were segregated for separate study.

In the case of the 4 *S*s only half of the observations were made indoors. The results of the 12 indoor records were separately compiled and, to provide comparable

data, a separate compilation was made of the 12 odd-numbered records of the *Ss* who were observed indoors exclusively. One *S* was transferred from one kindergarten to the other after 12 records had been taken, so she appears only once in the following table.

Where so few *Ss* are involved the results lend themselves better to detailed study of individual cases than to statistical treatment. As a basis for discussion, however, correlations were obtained between the records in this study and the *S*'s records of the previous year. The coefficients, obtained by the rank difference method, follow in Table I.

TABLE I

CORRELATION BETWEEN 1931 AND 1932 SCORES

Behavior	No. 5-min. observations		
	24 with 10 *Ss*	12 indoors, with 11 *Ss*	24 with 7 *Ss* belonging to 1932 group
(1) Use of material	.36	.30	−.07
(2) Contact with material (inactive)	.58	.39	.64
(3) Contact with material, (1) and (2)	.52	.51	.21
(4) Physical activity without material	.37	.34	.00
(5) No overt activity	.38	.47	−.11
(6) Total physical activity, (1) and (4)	.38	.23	.94
(7) Total no overt activity, (2) and (5)	.61	.35	.79
(8) Talking to persons	.19	.08	−.07
(9) Talking to self	.40	.45	.10
(10) Total talking	.28	.33	.32
(11) Physical contacts initiated	−.11	−.10	−.25
(12) Physical contacts received	.35	.75	.27
(13) Laughter	.11	.21	.05
Median	.37	.34	+.10

The coefficients above suggest that there is a small degree of conformity between the *Ss*' behavior of this year and last. Most of the coefficients are low, and, because of the small number of cases, unreliable, but they tend to the positive side.

The data were examined to find whether any uniform factor, such as a consistently high deviation on the part of only one or two *Ss*, was responsible for the low correlations. This examination left the results much as they were. In each correlation table there were *Ss* who deviated considerably from their rank of last year, but in the tables as a whole many different children were responsible for the high deviations.

The correlations here obtained correspond quite closely to findings reported by Arrington. Sixteen children were studied by means of her technique during two separate years while the children attended the same nursery school. The correlations between their scores in 1930 and in 1931 ranged from −0.22 to +0.62, with a median (crying excluded) of +0.28. Even under conditions as uniform as continued attendance at the same nursery school the behavior of children, as studied by this technique, shows little constancy.[3]

[3] Nine of the present children had been studied by means of this technique during each of two preceding years. The correlations between their records of 1930 and their records in this study ranged from +0.55 to −0.86, with a median correlation of +0.005.

Observations of the *Ss*, independent of the technique, supplemented by case studies and teachers' reports, revealed items which help to account for the low correlations. In some instances the *Ss* deviated widely from their previous scores by reason, apparently, of a change of companions. In other instances the change in scores could be attributed to a change in interest in play material. Two *Ss*, for instance, received high scores in physical activity, and in use of material, by reason of constant play in the sand-box at the nursery school. Their movements in the kindergarten were more versatile and vigorous, but this change in the content of their activities was not reflected in the scores. The procedure of recording in terms of units of time spent in an activity gave a higher score to continuous sedentary manipulation of sand than to vigorous, but not so continuous, play with heavy objects and apparatus.

A comparison between the *Ss*' relative scores in the present study and their scores of the previous year suggests that the behavior studied by means of the observational technique is largely a function of the particular situation and the particular individuals surrounding the child. The fact that most correlations are positive, although low, indicates that there is some degree of constancy in the behavior which differentiates one child from another. But in view of the low degree of correspondence between the records of two years it appears that the records reflect mainly the episodic interests and adjustments of the child rather than basic symptoms of his personality.

Apart from the observational technique, many similarities and some differences could be discerned in the behavior of the *Ss* as compared with their behavior of the previous year. One *S* who was so aloof and unenterprising in the nursery school as to be quite a distinguished character appeared to be a changed creature a year later in the kindergarten. Where once he mumbled softly to himself he now conversed freely with others, where previously he played alone, carrying in his hand a little stick or button, he now was energetic in the use of equipment and in joining in the play of others. In contrast to his marked change, several *Ss* showed characteristics which were observable the previous year. One *S* who frequently played the part of a ring-leader in the nursery school continued to initiate group activities in the kindergarten. Other *Ss* who seldom took the lead in initiating projects or in influencing the activities of others in the nursery school continued to play a rather inconspicuous and subordinate rôle in their new environment. One *S* who was active and alert, but quite independent in her activities in the nursery school continued her solitary enterprises the following year in the kindergarten. Many features of this kind, which could, perhaps, be reduced to quantitative terms, indicate the need for a more discriminative device for the study of children's behavior.

As far as the quantitative results of the present study are concerned there seems to be little constancy in the behavior of a child, as compared with other children, from year to year. But the foregoing discussion suggests at least two limitations in the technique by means of which the quantitative results were obtained. First, we cannot conclude concerning a child's characteristics simply by observing him in a particular environment in the company of a particular group of associates. His adjustments are likely to be quite specific. To obtain an adequate picture of his personality, with a view to inquiring how constant his characteristics are likely to be from year to year, it seems that it is neccessary to

observe him in a wide variety of situations. This point is raised by Goodenough in a study which made use, in part, of separated short samplings of children's spontaneous behavior.[4] She observed that a change in situation does not affect all children in a precisely similar manner, and that to obtain a truly valid measure of behavior observations must be taken "under such a diversity of circumstances as to constitute a representative sampling of the child's daily life."

Another limitation of the technique is apparent from the comments above. Simply to record that a child spends 5 sec. in contact with material, or that he has so many contacts with other children during a 5-min. period, leaves out many significant details as to what the child actually is doing. It is important to know something concerning the vigor of his actions (for example, is he merely fingering a small stick or is he handling large blocks and building a house?) and to know whether his contact with others is that of hitting, or joining in play, or the like. Until more is recorded concerning the content of his behavior, the record of his behavior will be quite threadbare. This question as to the validity of the measurements used in her technique is discussed by Arrington. From the observations made in the present study it would seem that unless more of the content of the child's behavior is taken into account the records will not have much significance. On the other hand, as soon as an effort is made to obtain a more adequate description of the child's behavior it is likely that the technique will be less reliable, as measured by agreement between observers. The present study tells more concerning the limitations of the technique that was used than concerning the general problem of the constancy of children's behavior patterns.

Summary

A technique for recording children's overt activity, use of objects, contacts with other children, laughing and talking during free play was used in a study of 11 kindergarten children who had been studied the previous year in the nursery school. Twenty-four 5-min. observations, distributed over several weeks, were made of each child. These data were supplemented by independent observations and case studies.

The quantitative records showed a somewhat low degree of conformity to the relative position of the same children in their scores of the previous year. It appeared that the records obtained during free play in the nursery school gave a measure of adjustments to a particular environment and to particular persons rather than an indication of basic behavior tendencies in the children. Data obtained apart from the formal technique showed that one child had changed his conduct in a decided manner, while several others continued to show characteristics which distinguished them in the nursery school. The study shows that to obtain indications of a child's characteristics, as expressed in his conduct, it is necessary to record the content and scope, rather than simply the duration and frequency, of his activities in a large variety of situations.

[4] F. L. Goodenough, Inter-relationships in the behavior of young children, *Child Develop.*, 1, 1930, 46.

CHANGE IN AFFECTIVITY WITH REPETITION

By E. M. VERVEER, H. BARRY, JR., and W. A. BOUSFIELD, Tufts College

The experimental evidence relating to the influence of repetition on affective judgments is apparently conflicting. Certain experimenters have found an increase in pleasantness to occur as a result of repeated presentations. Included in this group is Meyer[1] who worked with melodies and harmonies. Martin,[2] when studying the effect of prolonged observation of a picture, found affective value to decrease. Troland[3] states that continued repetition of any piece of music renders it substantially non-affective. Other observers have noted that repetition of a stimulus might either raise or lower the degree of pleasantness (Fechner[4]).

Gilliland and Moore[5] found increased liking for classical music, but very little change with 'jazz.' Ortmann[6] mentions both the change from unpleasant to pleasant with increasing familiarity in certain instances, as well as diminished liking with increased experience in others. Downey[7] found that most of her musical pieces became more pleasant after five repetitions, while one became less pleasant. Washburn[8] mentions that repetition may either raise or lower the pleasantness of a selection. In apparent contrast with the foregoing are the shifts of pleasantness and unpleasantness occurring as a result of the operation of Beebe-Center's Law of Affective Equilibrium.[9]

The present study was initiated in an attempt to discover further data regarding the precise relationship between repetition and affectivity.[10]

PROCEDURE

It is apparent that consideration should be given to the various temporal intervals intervening between the presentations of stimulus patterns. The procedure adopted has the advantage of combining the method of a series of continuous repetitions at one sitting with the method which allows a relatively long intervening period of a number of days.

* Accepted for publication November 1, 1931.

[1] Max Meyer, Experimental studies in the psychology of music, this JOURNAL, 14, 1903, 474-475.

[2] L. J. Martin, An experimental study of Fechner's principles of aesthetics, *Psychol. Rev.*, 13, 1906, 182-186.

[3] L. T. Troland, *Fundamentals of Human Motivation*, 1928, 285-296.

[4] G. T. Fechner, *Vorschule der Aesthetic*, II, 1876, 240-243.

[5] A. R. Gilliland and H. T. Moore, The immediate and long-time effects of classical and popular phonograph selections, *J. Appl. Psychol.*, 8, 1924, 309 ff.

[6] O. Ortmann, *The Effects of Music*, 1927, 44-66.

[7] J. E. Downey and G. E. Knapp, *The Effects of Music*, 1927, 227-229.

[8] M. F. Washburn *et al.*, *The Effects of Music*, 1927, 203.

[9] J. G. Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 64.

[10] The writers wish to acknowledge their indebtedness to Dr. Beebe-Center for suggestions and advice in the preparation of this report.

The experiment consisted in the playing of two 'jazz' selections[11] on a victrola to groups of not over 7 *Ss* at one time, all undergraduate students in psychology, who were placed so that there were no less than three intervening seats between them. The music was clearly audible to all. At the beginning of each series of observations, *E* read the following instruction: "I am going to play a few records for you. While each piece is being played, kindly keep your eyes closed and try to concentrate on the music as much as possible. At the close you will rate each piece on a +10 to −10 basis, according to the degree of pleasantness the piece has for you." At the conclusion of each selection, introspections were recorded and then the same selection (or a control selection) immediately followed. For the first group of *Ss*, selection No. 1 was played repeatedly, with selection No. 2 inserted after three or more repetitions as a control. After the control piece had been rated as to pleasantness, the cycle was repeated until the conclusion of the experimental period, which lasted over an hour. The experiment was continued with the same *Ss* and same procedure after an interval of one week.

For the second group, the procedure was similar, except that selection No. 2 was played repeatedly with selection No. 1 used as a control. Only one *S* had previously heard selection No. 1, while everyone knew selection No. 2. Both pieces were 'jazz' music and piece No. 1 was chosen for the amount of internal repetition which it contained.[12]

Results

A statement of the results is given in Table I. The successive presentations of the selections are represented by the figures 1 to 8 for the first and second weeks respectively. The absolute judgments of affective value are represented as deviations from the first trials. Values for the controls are also given in terms of deviations from the ratings of the first controls (Table II). It is plain from Tables I and II that affective value tends first to increase to a maximum and then to decrease to a value far below the initial value. If these data are plotted, the curves evidently are inflected. These changes in direction or affective 'inflections' characterize the data for the two selections with their controls.

The data of Table I show clearly the change in affective value for the two continuous series, also the changes resulting from the intervening interval of one week. The values for the controls over the same periods are shown in Table II.

Discussion

The experimental data here presented appear to confirm the following observations of Fechner: "With a continuous (or repeated) stimulus, the affective value increases up to a certain limit which can be designated as the ascending effect. . . . If the stimulus is continued or repeated past the point of optimum affectivity, its value is lessened, . . . unless there is an intervening time interval."[13] In other words, Fechner clearly distinguishes between the effects of continuous presentations and the effects of a relatively long intervening time interval. The results mentioned for Meyer,[14] Martin,[15] Downey,[16] and others seem reconcilable to Fechner's principle

[11] No. 1: *Lazy Levee Loungers,* Columbia, 14555-D (150660); No. 2: *What's the Use,* Brunswick, 4810.

[12] These results have been confirmed with a different set of records.

[13] Fechner, *op. cit.,* 240-243.

[14] Meyer, *op. cit.,* 474-475.

[15] Martin, *op. cit.,* 182-186.

[16] Downey and Knapp, *op. cit.,* 227-229.

of the ascending effect. Since stimuli may be represented as patterns of varying degrees of complexity, Washburn's results,[17] which indicate that the optimal degree of pleasantness occurs more quickly with popular than with classical music, and in general more quickly with musical than nonmusical observers, may be similarly interpreted. Gilliland and Moore[18] corroborate Washburn with respect to the influence of repetitions on classical music and jazz; Schoen and Gatewood[19] also make similar observations. Ortmann[20] finds both short (0.1 sec.) and long (15 sec.) tones are less pleasant than those of intermediate length (3 sec.). He also states

TABLE I

AFFECTIVE VALUE OF MUSICAL SELECTIONS WITH REPETITION

(Average degree of pleasantness +10 to −10 as a basis)

Selection:	No. 1 (8 Ss)	No. 2 (8 Ss)	No. 1 Extremes (3 Ss)	All Ss
Av. Trial No. 1	−1.63	4.38	3.67	1.74
Trial No.		Change from Trial No. 1		
1	0	0	0	0
2	.75	.25	0	.42
3	− .13	− .25	− 1.67	− .42
4	− .63	−1.13	− 2.67	−1.16
Control 1				
Rest				
5	− .38	− .38	− 6.33	−1.32
6	− .75	−2.13	− 8.00	−2.48
7	−1.38	−3.63	−10.33	−3.74
8	− .38	−4.75	−12.00	−4.05
Control 2				
2nd week				
1	1.75	− .25	.33	.68
2	1.63	− .63	− .67	.32
3	.88	−1.75	− 2.33	− .73
4	1.00	−2.00	− 2.67	− .84
Control 3				
Rest				
5	.38	−1.75	− 3.67	−1.16
6	− .13	−2.88	− 2.67	−1.69
7	− .75	−4.50	− 3.67	−2.79
8	−1.75	−6.75	− 3.00	−4.05
Control 4				

that chords which are at first decidedly unpleasant grow less unpleasant as they are heard repeatedly, and that in time musicians frequently change their feeling toward modern harmony from extreme unpleasantness to pleasantness.[21]

From the foregoing remarks it may be noted that (1) both selections indicate a change in affective value for a small number of repetitions up to an affective peak; that (2) following the affective peak, pleasantness diminishes progressively with further repetition,[22] and that (3) there is a tendency for the pleasantness of a selection to increase after a period of rest.

[17] Washburn, *et al., op. cit.,* 205. [18] Gilliland and Moore, *op. cit.,* 309 ff.
[19] M. S. Schoen and E. L. Gatewood, *The Effects of Music,* 1927, 171, 179-181.
[20] Ortmann, *op. cit.,* 44. [21] *Ibid.,* 44-66.
[22] The experimenter, who listened to the selections the largest number of times finally developed a headache whenever the selections were played.

It may also be seen that many of the experimental results which show a decrease in pleasantness due to repetition may be interpreted in terms of the second of the observations of Fechner quoted above. Mention may be made in this connection of the fall in pleasantness of familiar or relatively simple pieces mentioned by various experimenters, like Ortmann,[22] who observes that many musicians have seen the charm of their favorite pieces grow less with experience. It is possible that the apparent discrepancies noted by certain investigators may be due to a different time interval at which the affective peak is reached with varying complexity of stimulus pattern. There still remain to be reconciled the cases coming under Beebe-Center's Law of Affective Equilibrium,[24] supported by experiments performed by him, Herring,[25] and Pepper,[26] all showing a general tendency for both pleasant and unpleasant stimuli to become relatively neutral or indifferent. Apparently the principle implied in Beebe-Center's law of affective equilibrium and Harris' law of contrast (a principle subsumable under the law of affective equilibrium) may have superimposed upon it the principle of affective 'inflection' which is presented in this paper.

TABLE II

CONTROLS

Selection:	No. 2	No. 1	No. 2	All Ss
Av. Control No. 1	4.13	−2.75	4.00	1.21
Trial No.		Change from Control No. 1		
1	0	0	0	0
2	.38	− .25	−6.00	−.89
3	.88	− .88	2.00	.32
4	2.00	−1.63	.33	.21

If we wish to identify temporal repetition as a special case of increased intensity (with the addition of a temporal factor), the principle mentioned might be related to Troland's principle of the hedonic calculus,[27] for which the parts relevant to this discussion appear to have some experimental verification in the work of Ortmann[28] that shows that tones of moderate intensity are more pleasant than those which are extremely soft or extremely loud. Troland[29] and Engel[30] observe that various gustatory stimuli have optimum pleasantness when of moderate intensity. If the influence of increased intensity is similar to that of repetition, the principle of affective 'inflection' seems also to have been anticipated by Wundt[31] who stated that with increasing intensity of sensation, feeling tone becomes at first more pleasant until an optimal intensity of sensation is reached, after which the feeling tone becomes progressively more unpleasant. His curve, which was systematized from casual observation (since he says feeling cannot be measured precisely), resembles those which may be plotted from our data.

In support of the general thesis of this paper, attention may be called to the

[22] Ortmann, *op. cit.*, 66.
[24] Beebe-Center, *op. cit.*, 64.
[25] J. P. Herring, The measurement of liking and disliking, *J. Educ. Psychol.*, 21, 1930, 159.
[26] S. C. Pepper, Changes of appreciation for color combinations, *Psychol. Rev.*, 26, 1919, 389-396.
[27] Troland, *op. cit.*, 308-314.
[28] Ortmann, *op. cit.*, 44.
[29] Troland, *op. cit.*, 308-314.
[30] R. Engel, *Psychology*, 1928, 1-36.
[31] W. Wundt, *Grundzüge der physiologischen Psychologie*, 5th ed. II, 1902, 311-312.

following well-known phenomena: (1) the liking of children for the same familiar story, always in the same identical words; (2) the extreme unpleasantness of a once 'popular' tune after too extensive repetition; (3) the usual rise and decay of various fashions and styles; (4) the progress of musical preferences in the race and the individual from the simple to the more complex; (5) certain instances in which the economic law of diminishing returns are based on a purely psychological foundation.

Conclusions

(1) Fechner's observations on affective change with continuous repetition are confirmed.

(2) With closely spaced repetition, pleasantness tends to increase to an affective peak at an optimal degree of familiarity.

(3) Further repetition tends to make the experience progressively less pleasant.

(4) An intervening time interval, however, tends to enhance the pleasantness of subsequent repetitions.

(5) Apparently discrepant results of different investigators may be harmonized, at least in part, in terms of the contrary effects of continuous repetition and repetition at intervals. It has been demonstrated that Fechner's law of repetition applies—as was stated by Fechner—to cases involving continuous repetition. It is likely that Beebe-Center's law of affective equilibrium, or the habituation effect described by Herring, apply to cases involving repetition at intervals.

APPARATUS

A DEMONSTRATIONAL TEST OF VISION

By M. LUCKIESH and FRANK K. MOSS, Nela Park, Cleveland, Ohio

It is apparent to those engaged in research in the field of physiological optics that human vision does not benefit as it very well might from the knowledge thus gained. Due to the vagaries of human behavior, it is exceedingly difficult, *e.g.* to demonstrate the beneficence of more light or better lighting in a practical and convincing manner.

After a number of years of continuous experimentation, the authors have been able to present for the first time a simple and reliable method for demonstrating the influence of illumination-intensity upon visual efficiency.[1] Although the test described here was designed primarily to serve as a practical demonstration of the beneficence of better lighting, it has also proved to be an excellent research device, comparable in laboratory usefulness to far more complicated apparatus and experimental technique. It is applicable to, and has been successfully used in, a number of psychological researches.

A fundamental requisite of such a test demands that it respond significantly to relatively small changes in illumination-intensity in comparison with the enormous range of intensities under which the visual sense may function. Furthermore, it must be responsive to changes in illumination-intensity which are within the range actually encountered in lighting practice. Finally, any test-method which will be useful as a demonstration must be simple in operation and must yield positive results in all or nearly all cases in which it is tried.

In developing our instrument the experience gained in many behavioristic experiments was invaluable in providing certain limitations and suggesting certain methods of approach.[2] This experience indicated (1) that the instrument must be of the integrating rather than of the indicating type since a single observation on human behavior has little or no reliability as a quantitative datum. It was also recognized (2) that the test must operate at or near the differential threshold of visual ability in order to isolate the effects of small changes in seeing conditions by a test of short duration. A third fundamental requirement (3) necessitates some means for maintaining the effort expended under test as nearly constant as possible for various changes in the experimental variable. Without this control, the effect to be determined is often obscured, principally on account of the facility of the subjects in adapting themselves to widely different conditions, even at the expense of greater strain, effort, and fatigue. The fulfillment of this last fundamental requirement was the most difficult.

[1] From the Lighting Research Laboratory, General Electric Company.

[2] See the volume *Seeing; A Partnership of Lighting and Vision,* 1931, by the present writers, which contains the complete demonstrational test herein briefly described.

Description. The total time required to perform a series of exacting visual tasks was selected as the criterion of visual efficiency. The individual test-objects required the recognition of a capital letter formed by small breaks in parallel diagonal lines printed in dark gray upon a lighter gray background, as illustrated in Fig. 1. (In the illustration the pattern has been reproduced on a white instead of a gray background in order to show the test-object more clearly.)

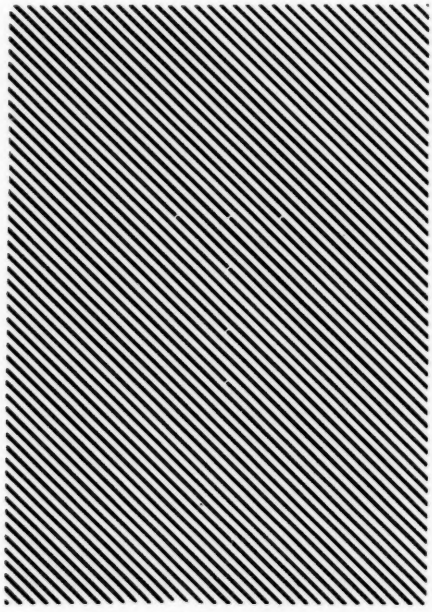


FIG. 1. THE TEST-OBJECT
The test-object is formed by breaks in the parallel-diagonal lines. On this pattern is the letter "T." For this illustration the visibility of the test-object has been increased by the use of a white instead of a gray background of 16.3% reflection-factor as used in the research form of the test.

The problem of producing a test-object of approximate liminal difficulty was accomplished by a combination of two methods. The first step consisted of making the breaks in the parallel diagonal lines fairly small. These breaks are approximately 0.3 mm. in width and subtend an angle of nearly 3 min. at the normal reading distance of 14 in. Due to the wide differences in visual acuity among various subjects, it was not considered advisable to approach nearer the actual visual threshold

by a further reduction in the size of the small breaks which formed the letter to be recognized.

The actual threshold of visibility was approached by reducing the contrast between the parallel diagonal lines and the background. The reflection-factors of the lines and background are 10.4% and 16.3%, respectively. Therefore, the contrast between lines and background is 36%, as computed by the formula: Contrast % = 100[(B.B. — O.B.)/B.B.], in which "B.B." is the background brightness,

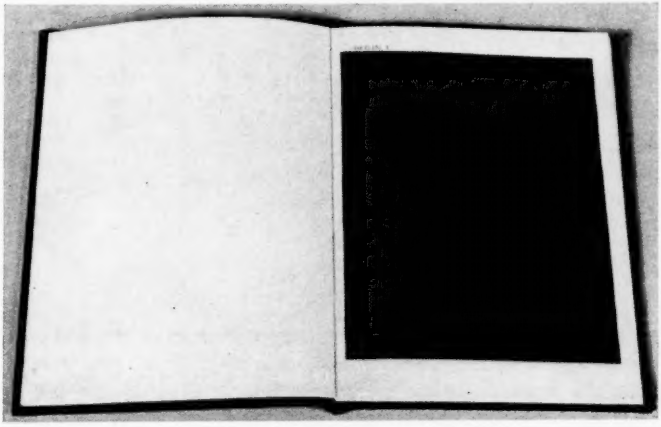


FIG. 2. TEST-BOOK OPENED AT FIRST PLATE
One of the 17 plates composing the series of test-objects. Each plate is composed of 4 patterns similar to that shown in Fig. 1.

and "O.B." is the object brightness. The visual task thus presented to the subjects was of sufficient difficulty to produce significant changes in the time required to recognize a series of 52 such test-objects for relatively small changes in illumination.

The purpose of the pattern of parallel diagonal lines is not to present a maze in which the test-object (the capital letter) is hidden. The letter has purposely been made large and as simple as possible while the factors which tend to make its recognition difficult (excepting the visual factor) have been minimized. The uniformity of the parallel lines makes it quite difficult for the eyes to remain fixated on a definite part of the pattern. As a result, normal eye-movements are greatly accelerated. This fact will be readily appreciated if Fig. 1 is viewed steadily for a few moments. The significant fact is that these eye movements are performed involuntarily and, since the subject has so little control over the amount of visual effort expended, changes in the experimental variable produce corresponding changes in the rate of working.

It will also be noted from viewing Fig. 1 steadily that definite color-effects are produced. It seems reasonable to expect that these color-effects are produced as a result of the rapid eye-movements over the stationary black and white pattern.

This effect may be analogous to the color-effects produced by a rotating Benham disk.

Performance. The task is to recognize the capital letter in the center of each pattern, of which there are 4 on each page (see Fig. 2) and to name it aloud to insure careful performance of the test. The first series of patterns begins on a page marked "Begin 1" and is concluded on the page marked "Stop 1." The series includes 52 patterns containing 13 different letters between these designations. The time required to perform this series is recorded. The experimental variable

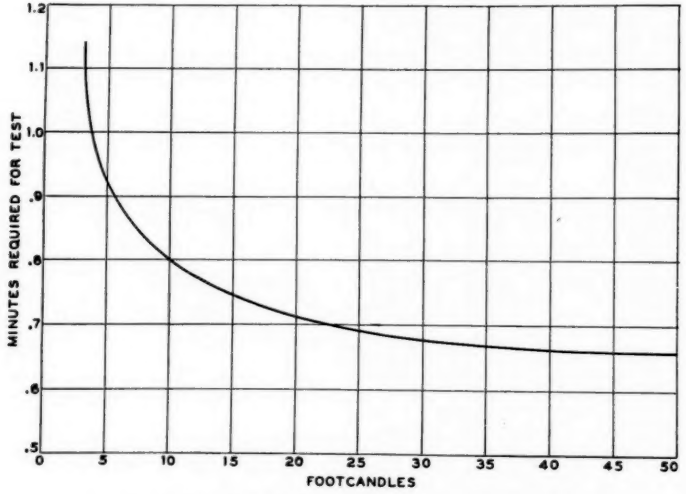


FIG. 3. TIME REQUIRED TO PERFORM THE TEST CORRELATED WITH INTENSITY OF ILLUMINATION

is then changed and the experiment repeated, beginning the second series on the page marked "Begin 2" and concluding it on the page marked "Stop 2." A third test should be performed under the same conditions as the first and the time required to complete the first and third tests averaged and compared with that of the second series.

The average results of a large number of tests in which illumination-intensity was the experimental variable are plotted in Fig. 3. It will be noted that the test is satisfactory for the range in level of illumination for which it was designed.

The influence on human behavior of other experimental variables may be readily determined by using this test-method as a criterion. For example, the test was conducted repeatedly under two different conditions: (1) in a quiet room and (2) in a room in which several small motor-generator sets were operating. The visual work was done in a booth and at a level of illumination of 5 foot candles in both situations. Hence the distracting noise of the machines is the only known variable in the experiment. The results follow:

Quiet room .....0.77 min. required per test
Noisy room .....0.82 min. required per test

These results indicate that the distracting noise of the machines increased the average time required for the test by about 6%, a difference more than 6 times the probable error of the measurements.[8]

SPECIAL APPARATUS IN THE LABORATORY OF EDUCATIONAL PSYCHOLOGY, UNIVERSITY OF TEXAS

By B. F. HOLLAND, University of Texas

EYE-MOVEMENT CAMERA

The eye-movement camera was built by C. T. Gray in 1915. Its essential features are described in his *Deficiencies in Reading* (179-182). In recent years the following improvements have been made by the writer, the essential features of the camera being left intact.

(1) A set of photographic lenses of 12-in. focal length was added to replace a set of lenses of 8-in. focal length. These lenses are known as the Wollensak Velostigmat, Series IV, f. 6.3 Anastigmat. They function best when the eye is about 14 in. from the front of the lenses. The particular advantage of these lenses over those of shorter focal length is that they make possible the use of longer lines and longer selections of materials.

(2) A metal head-rest, attached to the camera table by means of clamps, gives greater rigidity than Gray's wooden piece.

(3) The arc-light used by Gray was replaced with a Bausch and Lomb illuminating unit equipped with a ribbon filament lamp of the 6-volt, 108-watt type, and a spherical condenser with rack-and-pinion focusing mount. The light produced by this unit is satisfactory for photographic purposes. After the unit is properly adjusted, it requires little or no further attention.

(4) A lighting device was designed to illuminate the reading material. This mechanism includes a regular automobile dash-light unit and a sheet-metal projector. The projector is a rectangular tapering inclosure attached to the lighting unit, being soldered to a metal cap which covers the dash-light bulb. Its dimensions at the source of light are ½ in. × ⅜ in. and at the large end from which the light emerges 2 in. × 3½ in. The total length is 7 in. The entire unit is attached to the upper cross-bar of the head-rest by means of a clamp. It can be adjusted to illuminate a small or a large amount of reading material. The lamp receives current from the city 110-volt line through a transformer.

[8] The printing of this visual test was in itself a research-problem requiring photometric measurements and necessary alterations in the reflection-factors of the inks at each of the several stages in printing. For this reason it is obviously impossible to reproduce the test-pages in this publication. Copies of the test to be used for research purposes will be supplied at a nominal price by Lighting Research Laboratory, General Electric Company, Nela Park, Cleveland.

(5) A film mechanism (see Fig. 1) constructed to carry standard moving-picture film replaced one suitable for kodak film only. The mechanism was designed by the writer and built by W. L. Benson, University mechanician in Chemistry. Four film-boxes are arranged in opposite pairs, the boxes at the top and left holding a supply of unused film, the other two receiving film as it is exposed. The film is pulled from the supply-box by means of a sprocket which operates in the inclosure just above the lower film-box. This sprocket is operated on a shaft shown just under the left film-box, the shaft being turned by a pulley belted to a speed-

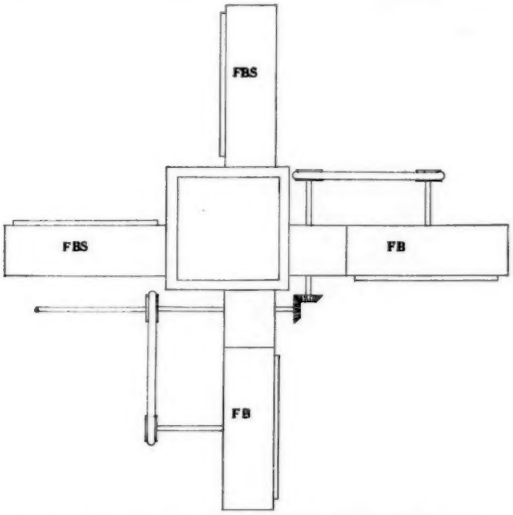


FIG. 1. BACK VIEW OF EYE-MOVEMENT CAMERA FILM MECHANISM

reducing unit which is operated by an electric motor. Another pulley on this shaft is connected to a pulley which operates a core in the film-box on which the film is wound as it comes from the supply-box. These pulleys are articulated by means of a steel-spring belt.

Fig. 2 is a sketch of the mechanism as seen from the left side. (FBS) is the film-supply box, (F) the film, and (S) the traction-sprocket. The unexposed film is placed in (FBS,) pulled through a narrow slit in the film-box, run across the exposure-box (LB), and drawn around the sprocket (S) from which it goes to the core (C) in the film-box below. As it turns the sprocket pulls the film across the exposure box, being held in place by a system of rollers (R). Behind the film in the exposure-box is a plate of ground glass (G) upon which the light from the subject's eye is brought to a focus to be seen and regulated by using pulleys of various sizes on the motor or the speed-reducer. The film moves about one foot in 3 sec.

The entire film-mechanism is mounted on a structure which connects with the camera tube by means of a photographer's bellows. This bellows enables the operator to disengage at will the mechanism from the power unit. Thus it is possible to present a subject with several practice selections with all of the moving parts in operation except the film-mechanism, the subject not realizing that his eyes are not being photographed. When the experimental selection is ready to present, the operator moves the mechanism backward until, the belt becomes taut and the film is set in motion. In actual experimental procedure, this feature of the set-up has distinct merit.

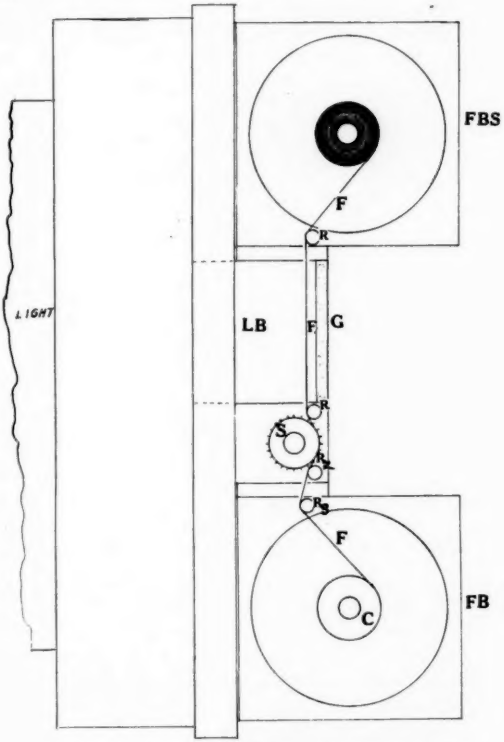


FIG. 2. SIDE VIEW OF FILM MECHANISM

AN EXPOSURE APPARATUS

The exposure apparatus was built about a screen manufactured by the Error-No Company, Inc. of Rochester, N.Y., to hold a stenographer's copy while working at a typewriter. To convert the mechanism into an exposure apparatus for experimental or laboratory work, it was necessary to make several changes.

Fig. 3 shows front, back, and side views. The carrier-plate (A) to which the copy of material is attached by the clamp (B) is raised intermittently by the wheel (C). A metal tape, attached at one end of wheel (C) and drawn over the stationary pulley (E) to the lower edge of carrier (A), where it is fastened, is wound about the wheel (C) as the wheel rotates and thus raises the carrier-plate. Wheel (C) is turned by the friction-drive-wheel (G) in mesh with (C). Through the drive-wheel (G) is a shaft (H) to which is attached a ratchet (I) that is in turn articulated with a shaft fastened eccentrically to a pulley on the shaft of a chain of gears (K). The gears serve as a speed-reducing unit between the motor (M), being connected with M by means of a round belt and pulleys, and the carrier-plate. The pulleys on the speed-reducing unit, as well as those on the motor are of various sizes, so that the carrier-plate can be lifted at various speeds. This provides for a large range of exposure-times for material on the carrier.

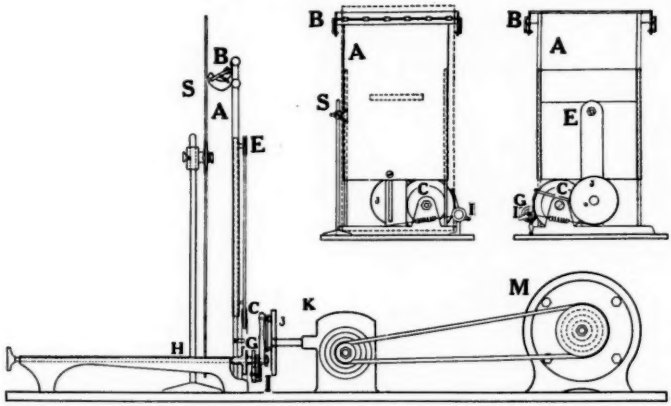


FIG. 3. FRONT, BACK, AND SIDE VIEWS OF EXPOSURE APPARATUS

Materials are exposed through a slit in the black screen S, cut at any desired length and height. The eccentric device is adjustable so that the amount of lift given the carrier can be varied. The adjustment enables the operator to space the lines to be exposed on a sheet with one, two, or three vertical spaces between. By using various lengths of slit any desired length of line up to eight inches is exposed. The carrier is lowered when it reaches the upper end of its lift by releasing the friction-drive-wheel (G) from wheel (C). The carrier drops by gravity and starts up again as soon as the wheels are enmeshed. The two wheels are held in mesh by a taut coil spring.

The changes in the Error-No necessary to convert it into the exposure apparatus involved moving the ratchet device from the front end of shaft (H) to the back end, as shown in Fig. 3. The shaft had to be adapted to this change. By using a synchronous motor, it is possible to expose materials at regular intervals of equal length, either simultaneously or successively. The apparatus has been

used in elaborate experiments by Dr. Helen L. Koch and for class learning experiments. The entire mechanism is mounted on a stand 16 in. × 36 in.

Apparatus for Photographing Finger Movements

A mechanism used to record the finger movements of blind persons in reading Braille or Point includes the following assembly: a moving-picture camera, a speed-reducing unit, a synchronous motor, lighting and projecting devices and supporting tables (Fig. 4). The camera (A) is known as the Model A Universal equipped

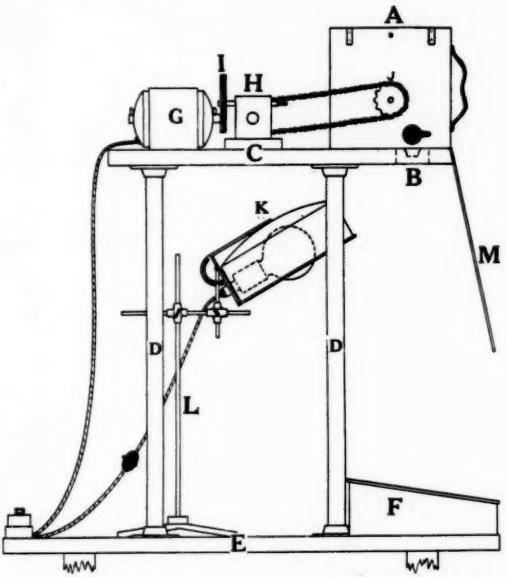


Fig. 4. Set-up for Photographing Finger Movements of Blind Readers

with a lens (B) of the constant-focus type. The camera is mounted on a platform (C) supported by four 1-in. pipes and flanges (D) to the top of a table (E). The lens points downward through an opening in the platform and is so arranged that the subject's fingers can be photographed while reading material placed on the reading desk (F) fastened to the table top. The camera is driven by a Westinghouse synchronous motor (G) which operates at a constant speed of 1800 r.p.m. Between the motor and the camera is a speed-reducing unit of the noiseless, run-in-oil type (H), made by the Boston Gear Works. This unit is driven by the motor by means of pulleys and belt (I). The camera mechanism operates off the slow shaft of the speed-reducer by means of a sprocket-and-chain assembly (J) which takes the place of the hand-crank with which the camera is equipped. Light

is furnished by a 1000-watt "Fotolite" (K) mounted on a stand (L) directly in front of the subject. A card-board screen (M) protects the subject from the glare and heat from the lamp.

In photographing finger movements, it is necessary to run the camera at such a speed that the subject can make no significant movements between exposures as he moves his fingers along the line of print. For ordinary purposes, 1/5-sec. exposures seem to meet this need. Since the constant-speed motor insures a regular exposure-time and a constant interval between exposures, the rate of reading by a given subject is determined by counting the number of exposures per unit of material (as word or line). By projecting the record on the material read, the position of the fingers at any point in the performance can be determined.

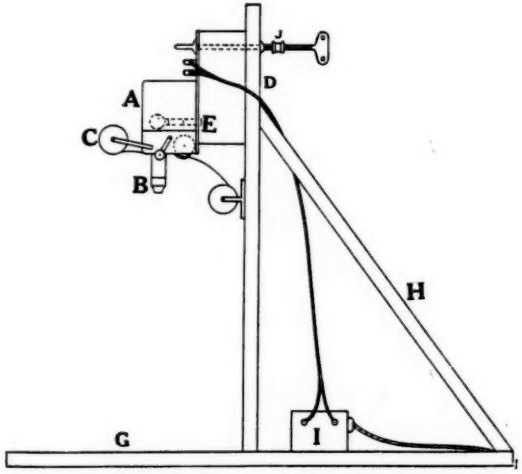


FIG. 5. DEVICE USED IN PROJECTING MOVING-PICTURE FILM RECORDS

PLOTTING DEVICE

The records obtained by the eye-movement camera or by the moving picture camera can be superimposed upon the material read by means of any projection lantern. The particular projector used at the University of Texas for this purpose is illustrated in Fig. 5. A light box 4 in. × 6 in. (A) contains a lens (B) and film mechanism (C) and is mounted on an upright (D) by means of the clamp (J). The upright piece is mounted on a board and held in rigid position by the support (H). The light-box contains a 32-candle power automobile light globe lighted by a 110-volt current through a transformer. By moving the light box up or down on the upright piece, the film record can be accurately focused and superimposed upon the material placed at (G). The block (E) holds the lens out from the upright in order to provide space for the sheet of paper containing the material.

The records of finger-movements are relatively easy to project if one observes the following procedure. First, the Braille characters used as experimental material are colored with black India ink. In the film-record the dots thus appear white; secondly, photostatic copies are made of the material with lines spaced about ¾ in. apart. These photostatic copies then contain black dots upon which the white dots on the film are projected. The film is moved one exposure at a time and a record is made of the position of the finger in that particular exposure. Thus one gets the number of exposures, or time, and the location of the fingers during each exposure.

SOME NEW ELECTRONIC DEVICES FOR THE PSYCHOLOGICAL LABORATORY

By DAVID P. BODER, Lewis Institute

I. A PHOTO-ELECTRICAL REACTION KEY

The device presented on Fig. 1 is a photo-electrical reaction key. (C) is a glass tube, ½ in. in diam., supported by metal ends such as are used for towel bars. Half-inch holes are drilled in the back of ends so that rod (E) can be introduced into the tube and also for purposes of ventilation. (D) is a 6-volt radio

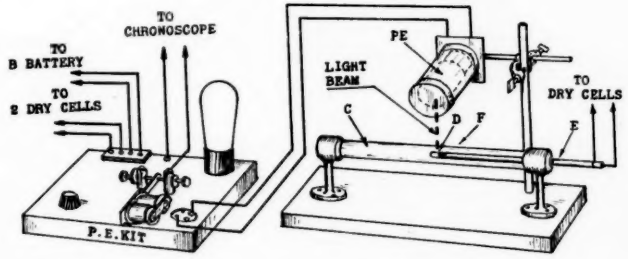


FIG. 1. A PHOTO-ELECTRICAL REACTION KEY

pilot light screwed into a socket, soldered to rod (E). The light from (D) activates the photo-electric cell (PE) which is covered by a cardboard pill box with a 1 × 1½ in. opening for the entrance of the light and is connected with a DeVry Photo-electric Kit. When the finger, placed at (F), is moved to the left, the light beam directed from (D) into the photo-electric cell (PE) is interrupted; this produces a break into the (PE) relay, and so breaks the chronoscope circuit, bringing the chronoscope handle to a stop. The reactions so far obtained compare favorably with, and are by no means longer than, those obtained with other chronoscope reaction keys, and the necessary number of practice trials for naïve subjects appears with this device significantly less than when other keys are used.

II. A Voice Key and Speaker Relay

A microphone is connected to an amplifier or to an ordinary radio set.[1] (A) and (B), Fig. 2, are two magnetic loud speaker units, viz. a Baldwin and a Temple unit. To the armature of the speaker (A) is soldered a fine "jeweler's blade" (A_1), and to the rod at speaker (B) is soldered the brass attachment (B_1). The adjustment is made in such a way that (A_1) and (B_1) make contact in spite of slight impulses coming from the amplifier (or radio set). The leads from both speakers, in parallel or series, are connected with the output of the amplifier. When the accentuated syllable is spoken into the microphone both speaker units respond to an extent that (A_1) and (B_1) throw each other apart and the circuit

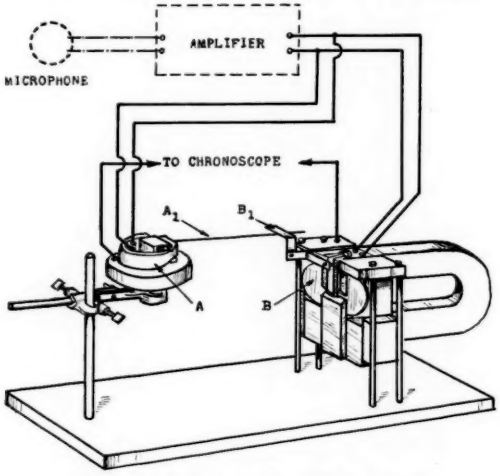


Fig. 2. A Voice Key and Speaker Relay

of the chronoscope is broken. This same circuit-breaking device can be used to activate a signal magnet and to record on a kymograph or polygraph each accentuated syllable spoken into the microphone. Some disadvantages lie in the fact that the speakers are rather noisy and proper measures for damping the noise through rheostats or soundproof casing is desirable. It seems that unit (A) alone, with the blade (A_1), could be used for photographic recording of the sound impulses from the microphone, although for these purposes the device described below may be more efficient.

III. A Photo-Electric Word Counter

To eliminate noise and other possible difficulties which presented themselves with the speaker relay, the apparatus shown in Fig. 3 was built. A neon television lamp

[1] Dr. Arthur Bills of the University of Chicago coöperated in the construction of this and the succeeding piece of apparatus.

(A) with a one-inch square screen is hooked up with the amplifier in place of the speaker, or in series with a magnetic speaker. The television lamp flashes with every accentuated syllable and so activates the photo-electric cell (PE) connected with the DeVry Photo-electric Cell Kit. The relay (R) of the Kit makes or breaks the circuit of the signal magnet (S) or any other writing device, offering the possibility for a study of temporal relationships between a number of syllables spoken in succession.

Other Uses of the Equipment

(1) A television lamp of larger size such as manufactured by the National Carbon Co. (Everready) substitutes admirably, at least for demonstration purposes, the acetylene flame for the Scripture disk, and promises simplification of stroboscopic studies such as with the Seashore Tonoscope.

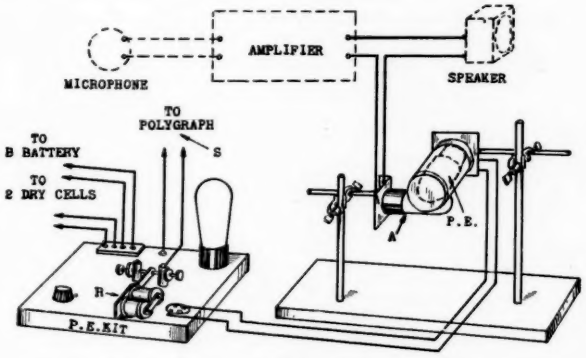


Fig. 3. A Photo-Electric Word Counter

(2) A photo-electric timer for ½ or ¼ sec. is easily constructed by adjusting windmill wings to the shaft of the starting gear in the back of an inexpensive electric clock. This clock is placed in such a way that the wings interrupt the beam of light directed from a flashlight bulb into the (PE) cell—arranged as in Fig. 3, substituting flashlight for (A). The choice of the number of interruptions appears to depend mostly on the sensitivity of the signal magnet of the writing device. At the Lewis Laboratories up to 6 interruptions per sec. were obtained, but it would be safer to use it for not more than 4 per sec.

(3) A constant gramophone motor can successfully be used instead of the clock. Cardboard strips about 5 in. wide protruding about 4 in. outside the revolving disk serve as excellent beam interrupters—arranged as in Fig. 1—the whole device makes an excellent time-marker free from mercury or other mechanical contacts, and operates almost noiselessly. The whole equipment is relatively inexpensive.

A SIMPLE CAMPIMETER

By FORREST LEE DIMMICK, Hobart College

The ordinary type of quadrant perimeter has several disadvantages for use in the elementary laboratory. The simpler forms require steadiness on the part of the manipulator and are correspondingly inaccurate; the better instruments are expensive; and all necessitate duplication of the observations if each student is to understand the experiment. A campimeter described by Titchener avoids some but not all of these difficulties.[1] Following it in principle, we worked out a device in the Hobart Laboratory that has been most satisfactory for the elementary laboratory experiment.

The apparatus (see Fig. 1) consists of a tripod head-rest which rests upon a table in front of *O* and holds his head horizontal 20 cm. above the campimeter field.

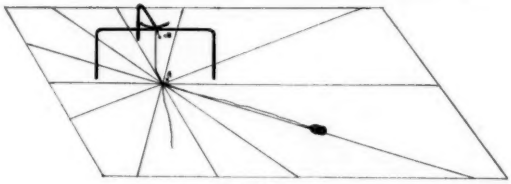


FIG. 1. THE HOBART CAMPIMETER

The head-rest has two eyelets, one (A) that just clears the table, the other (B) directly over the first and 1 cm. beneath the right eye, so that when the head is in position *O* fixates the lower eyelet through the upper. The stimulus, a disk of colored paper 1 cm. in diam., is carried on a slide made of thin metal (1 cm. by 2 cm.) painted gray. A thread is attached to the slide and runs through the lower eyelet. The student prepares a campimeter about 50 cm. by 75 cm. in size with lines 30° apart radiating from a point 25 cm. from the top and left end of the paper. The head-rest is placed with the lower eyelet (A) directly over the center of the radiating lines. *O* blindfolds his left eye and puts his head on the head-rest so that with his right eye he fixates through the two eyelets upon the focus of the lines. The stimulus-carrier upon which a disk of the color to be studied has been placed is set at the extremity of one of the lines on the chart, and *O* pulls on the free end of the thread bringing the stimulus slowly in along the radius upon which it is set until the color is seen. When this point is reached, he stops pulling and marks the position of the stimulus. Repeating this procedure for every one of the radiating lines he obtains a fairly accurate map of his field of vision for that color.

[1] E. B. Titchener, *Experimental Psychology, Student's Manual, Qualitative,* 1901, 10.

APPARATUS NOTES

Tables for the Constant Methods

I have recently had reprinted, for the convenience of students in psychophysics, the various tables that are used for computing and checking in the methods of constant stimuli and constant stimulus-differences.

Urban's table, reprinted from the *Archiv für die gesamte Psychologie*,[1] is reproduced with all the corrections that have been reported to date,[2] and with rules intersticed after every 5 lines so that the reader's eyes may more easily and accurately follow across the double page.

Rich's checking table is reprinted from this JOURNAL.[3] It is reproduced, with 5 corrections,[4] in a single double-paged table. The values for 'x' from —7 to +7 and at the different percentages can be read across and down the table without turning a page. The ease with which the table may be used is thereby greatly increased.

Hoisington's table for graphic checking, also reprinted from this JOURNAL,[5] is reproduced without change.

All the tables were reprinted by line cuts. No type was set; the corrections mentioned above were made in the original tables, which were then photographed and reproduced in zinc. Thus new errors in the reproduction were avoided.

Copies of these tables are available at a slight cost. They may be obtained from the Cornell University Department of Psychology or the Business Office of this JOURNAL.

Cornell University KARL M. DALLENBACH

A Useful Periodical on Instruments

Instruments, a monthly magazine, contains much information of interest to experimental psychologists. One of its most valuable sections is the department, "New Instruments," where the latest types of measuring, recording and controlling devices are illustrated and described. Examples of some of the instruments used by psychologists and described in this magazine are the following: Steady electric governed variable speed motors; Silent vacuum contacts; Illuminometers; Small relays; Photocell outfits; Stopwatches; Speed indicators and counters; Electric counters; Electric meters; Color matchers; Recording apparatus; Automatic regulation and control equipment; and Temperature control devices.

---

[1] *Op. cit.*, 24, 1912, 240.

[2] Four errors have been reported: two by F. M. Urban (*Praxis der Konstanzmethode*, 1912, 20), and one each by J. G. Rich (this JOURNAL, 29, 1918, 121) and H. H. Long (*ibid.*, 33, 1922, 303).

[3] *Op. cit.*, 29, 1918, 120 f.

[4] A typographical error noted by Rich (this JOURNAL, 29, 1918, 232), and four changes necessitated by Long's correction of Urban's table. Two of these four changes were pointed out by Long (*ibid.*, 33, 1922, 303) and two by H. F. Davies (*ibid.*, 34, 1923, 311). One of Long's corrections, as Rich later pointed out (*ibid.*, 33, 1922, 450), is incorrect; and so also, strangely enough, is one of Davies'. The value for $p = .27$ and $x = +2$ should be 4.9665, instead of 2.9665 as given by Davies.

[5] *Op. cit.*, 33, 1922, 245.

In each issue there are two or three articles on different phases of instrumentation and several pages of abstracts of articles on instruments in American, English and German publications.

In addition Mr. Richard Rimbach, its managing editor, offers to answer all queries of subscribers concerning the availability of different kinds of instruments. As I have met Mr. Rimbach and have made use of his information service I can testify that it is promptly, efficiently and cheerfully rendered.

Sample copies of the magazine will be sent on application to Instruments Publishing Company, 3619 Forbes Street, Pittsburgh, Pa.

Massachusetts State College HARRY R. DESILVA

A SIMPLE TACHISTOSCOPE OF MANY USES

Modern experimentation not infrequently demands that stimulus-objects of various sizes be exposed for brief and definitely determined lengths of time, and that the observer be maintained in a condition of light-adaptation between exposures. Since the ordinary tachistoscope is unsatisfactory in at least two of these respects, the writer has employed a modified mirror-tachistoscope with a condenser-charge control which has proved very efficient. The tachistoscope, based upon a general principle which Dodge (1907) cleverly adapted from a spectacular illusion of stage magic, is constructed of two chambers, one of which is set at right angles to the other. At the junction of the two chambers, a sheet of good glass is set at an angle of 45° to the line of regard. When the chamber beyond the glass and along the line of regard is dark, and the chamber at right angles to the line of regard is brightly illuminated, the glass serves as a mirror and reflects in excellent fashion whatever object (frequently a fixation-point) is put in this 'fixation' chamber. When the illumination is reversed with the fixation chamber dark and the exposure chamber illuminated, the observer is, of course, able to see directly through the glass which does not interfere at all with his view of the stimulus-object.

The writer will presently publish a wiring diagram of the condenser-charge apparatus designed by a local electrical engineer for the express purpose of the reciprocal operation of the lights in the two chambers. For the moment it may be enough to say that a condenser is charged through the grid of a tube, the plate of which is in series with the windings of a make-and-break telephone relay operating on 90 volts. The capacity of the condenser being held constant, its time of charge is a function of a resistance employed as a leak in the grid circuit, and the time may be varied without difficulty simply by varying the resistance. This is done in the present case by having available a series of grid leaks (1 to 20 megohms) which may be inserted in the circuit as they are needed. The apparatus is stable and is capable of operating for any length of exposure from several minutes down to a few sigma (the lower limit being set by the relay). Another virtue is that it may be constructed for almost any separation of observer and stimulus-object, as well as for any size of exposed object. The flexibility of the apparatus as to size, distance from fixation-point, and length of exposure, has made it a useful addition to the equipment of the Cornell laboratories.

Cornell University JOHN G. JENKINS

NOTES AND DISCUSSIONS

Gestalt Psychology and Behaviorism

Modern experimental psychology, having passed through two stages of development, seems about to enter a third. Unlike the so-called "natural" sciences, psychology has been unable to build a science of facts wherein each fact indicates the interpretation that should be put upon it, because the question as to what a psychological fact is still remains open. According to the Wundtian tradition a sensation is a psychological fact, and the various sensations can be classified with respect to their qualities and other variables. According to the Brentanian tradition it is not the content of experience but the experiencing, not the sensation but the sensing, that is factual. Thus a structural psychology differentiates itself from a functional psychology, the one finding its facts in the *contents* of experience and the other in the *acts* of an experiencing individual.

For purpose of experimentation the Wundtian position proved more fruitful than the Brentanian. One could study sensation in terms of the physical and physiological conditions of the sense-organ involved; whereas sensing is so general an activity that one can do little more than point to the thing sensed as being either a physical object or the result of some previous sensing of a physical object.

English psychology, which was slow to adopt the experimental method, shows the influence of Brentano, notably in the writings of Stout and Lloyd Morgan. American psychology, however, adopted the experiment at once, and laboratories were established in every college. But the experimental method employed in these laboratories was in many cases modified in the direction of a functionalism suggested by James, in which both contents and acts, things and their relations, existential data of consciousness and the relations of these data with one another, were accepted as two kinds of facts with which psychology might deal.

Thus the stage was set in America for behaviorism at the same time that Gestalt psychology was emerging from the traditions of Wundt and Brentano in Germany.

The American revolt against the Wundtian tradition of existential psychology and the functionalism of experienced things and their relations, was a revolt against the private character of experience as such. The German revolt against contents and acts was directed against the static character of a psychology in terms of sensations, affectations, images, and thought-contents such as *Bewusstseinslagen* and *Bewusstheiten*. American behaviorism began by translating sensation and sensing into the objective terms of stimulus and response, and obtained its objective character by the simple expedient of correlating stimuli which could be defined physically with reactions which in principle reduced to muscular contractions and glandular secretions. The living body, with its sense-receptors connected by nerves through spinal cord and brain with muscles and glands, was supposed to supply a frame-

* This paper was read at the Tenth International Congress of Psychology in Copenhagen, August 23, 1932.

work within which varied combinations of these connections by use and disuse would account for development and learning. Behavior, for this kind of behaviorist, was a more or less arbitrary combination of responses, each of a "can't-be-helped" type and thoroughly mechanical in its operation.

Gestalt psychology began by criticizing the structure of experience. Instead of denying consciousness as a means or end of psychological investigation, it pointed to the inadequacy of sensation and its derivatives, including the relations between contents, as a scientific description of experienced events. Wertheimer's "phi-phenomenon" of movement was a challenge to the existentialist because the phenomenon could not be reduced to the elements of experience with which existential psychology operated. While motor psychologists accepted it as evidence of a neurological short-circuit vaguely reflected in consciousness as a kinaesthetic experience that could not be precisely defined, Titchener set one of his students to study it *sub specie existentialis* with the result that it was defined as a *gray flash*.[1] But though all seen-movements might be "gray flashes," it was not apparent that all gray flashes were seen-movements. In other words, the meaning of the phenomenon was intentionally excluded from the existentialist's description. The issue here was one of principle. Titchener contended that there were no meanings in science. The descriptions of science, he said, deal with bare facts. Implications are logical and referential. But where do these implications arise if they have no basis in fact?

Gestalt psychology maintained, as James had done, that meanings are implicit in every phenomenon; that they are fundamental conditions of the experienced phenomena. Thus no phenomenon is significant *per se*, but is always a phenomenon of something else. The something else is a behavior, or a part of a behavior.

Thus Gestalt psychology became a behaviorism,[2] but it reached this conclusion by a different route and with a different orientation from those of American behaviorism.

The Gestalt psychologist, unlike the behaviorist, was not at first disposed to deny consciousness, but to reinterpret it. Instead of questioning the existentiality of consciousness as a fact, he questioned the existentiality of the elements into which consciousness had been analyzed.[3] Instead of "complexes" and "constellations" of elements with their attributive variables: quality, intensity, extensity, duration, etc., he found *Gestalten*, each of which was an unanalyzable whole—a phenomenon which analysis utterly destroyed, leaving in its place fragments which are no more the constituent parts of the original whole than fragments of broken glass are the true parts of a bottle to which they once belonged.

But this criticism of the "constancy-hypothesis" of the existentialist was only a beginning, which gradually led to a new psychology of perception. Hitherto perception had been a certain complex of sensations, tied imagery, and affections; now it became something over and above these elements in combination. A new content had been produced which was something more and other than these elements had their attributes. To the Austrian School of Psychology a *Gestalt* quality was a synthetic product. For the members of this school the elements remained ultimate facts, but the qualities of form, geometrical, musical and so on, were

[1] F. L. Dimmick, An experimental study of visual movement and the phi phenomenon, this JOURNAL, 31, 1920, 317-332.
[2] M. Scheerer, *Die Lehre von der Gestalt*, 1931.
[3] R. M. Ogden, Are there any sensations? This JOURNAL, 33, 1922, 247 ff.

seen to depend not on their elements but upon their relationships. Elements *exist,* but the relations which constitute a melody and permit its transposition on another key *subsist.* Analysis of a melody breaks the subsistent relations of its notes, and thus destroys it utterly, leaving only the elemental discrete sounds as fragments.

But what is the activity which thus binds elements together by creating subsistent relations between them? Science is a knowledge of facts. Agencies that bring things together and tear them apart must be known as facts before they can be accepted. Even though they are confined to one's own private consciousness, sensations, images, and feelings are in some sense facts; but the relation of two or more of these elements as they belong or do not belong together is nothing one can describe in terms of itself. The relation of one thing to another does not exist, it subsists between the two. The analytic facts are these two things, but their relationship is something more and other than the two facts taken separately. Consider a perception of two things, one greater than the other, without analysis. Is it possible to regard this perception as one whole in which the analytic facts of the two separate things have not yet emerged? Köhler's choice-training experiments with hens,[4] seemed to demonstrate that at a low level of discrimination the factual situation is not two things, one greater than the other, but a gradient of direction from less to more, or *vice versa.* At this level perception is not a complex of factual things; it is a direction of behavior which includes not only the two things as terminals of the direction—more or less than—but also the hen which wants food that can be taken from one terminal of this gradient. From such experiments we learn that perception is more than a complex of conscious facts with relations somehow added to make them wholes; it is a dynamic movement of the organism towards or away from certain features of the environment that attract or repel it.[5]

Thus Gestalt psychology, beginning with existential phenomena, was led to a behaviorism which denies the reality of these phenomena; not, as the behaviorist said, because they are private—mine and yours—but because they are at best criteria of behavior and not facts in themselves.

Meanwhile, the development of behaviorism has been moving in the same direction. The investigations of Lashley have shown the inadequacy of any theory of behavior which regards external stimuli as the motivating occasion for the reactions of an organism.[6] The frame-work of fixed connections in its nervous system gives but a partial account of the organism's behavior. In addition to the fixed connections which account for reflex action, a self-regulation of the organism from the very beginning of the individual's life, and throughout its embryonic development, has been amply demonstrated by the anatomical researches of Coghill.[7] The organism behaves, to begin with, as a fully integrated unit and proceeds in its development to throw off partial patterns of behavior by means of special organs which are individuated both structurally and functionally in response to organic needs.

The behavior of a living creature is neither predetermined by its inherited struc-

[4] W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. Preus. Akad. d. Wiss., Phys.-math. Kl.,* 1918, 101.

[5] Cf. the investigations of K. Lewin. Summarized in *Die Entwicklung der experimentellen Willens-psychologie und die Psychotherapie,* 1929; and in *Psychologische Situation bei Lohn und Strafe,* 1931.

[6] Cf. *Brain Mechanisms and Intelligence,* 1929.

[7] Cf. G. E. Coghill, The structural basis of the integration of behavior, *Proc. Nat. Acad. Sci.,* 16, 1930, 637 ff.

ture nor is it a result of chance-connections determined by environmental influences. The old problems of nature and nurture involving the major influences of heredity and environment are pseudo-problems. What the organism does at any moment of its existence is a result of such a structure as it may possess when animated by a want. This want finds its expression in the attractive and repellent features of its environment. Without the presence of a goal towards which the organism can strive, behavior lacks definition and is aimless. But the very aimlessness of a restless behavior alters the environment in which the organism finds itself, and thus creates the possibility of accents or foci of attraction and repulsion which give pattern to its behavior.

Thus Gestalt psychology and behaviorism converge upon the same problem with the same outlook. Many of us are no longer arguing whether we shall or shall not include the phenomena of consciousness in psychology, because we are agreed that the facts of psychology are facts of behavior. But we need not rule out the conscious criteria of behavior because they are private. They are that only so long as we regard them as facts in themselves. So soon as conscious phenomena are accepted as criteria of behavioral adjustments of the organism to its environment the nature of these phenomena no longer interests us as a structural fact but only as a functional index of the behavior that is taking place.

The behaviorist's first statement of his problem, with consciousness excluded, left him to pursue a generalized physiology of the whole man; a pursuit which did not clearly differentiate psychology from physiology. But psychology is something other than general physiology, its special province being the employment of conscious phenomena as criteria of behavior.

The step which modern psychology has at last taken, and which bids fair to define it as a natural science, is the frank admission that experience, mysterious though it may be as a phenomenon, is always an experience of behavior, or some aspect of behavior, and never an experience *per se*.

In accepting perception as the starting-point of any truly psychological investigation, we are dealing, not with a panorama of sensory phenomena, but with an adjustment of the living creature to its environment. This adjustment involves conditions which point in two directions: backwards and forwards. The backward direction leads us to investigate pre-perceptual adjustments of an instinctive character to which no special heed was given. Instinctive behavior is "blind" because its process of "closure," or achievement, does not permit the differentiation of the self which wants something, from the goal which satisfies this want. Whenever an attitude of receptivity or denial emerges as a partial pattern within the total behavior of being attracted or repelled by an environmental thing or event, we have perception. The creature then heeds its own actions in terms of this differentiation of its behavior pattern.

Having once felt this differentiation of its own wants from an object or goal that will satisfy these wants, the perceiving organism can proceed to gain insight into its behavior by a further process of individuation. The criterion of insight is a discernment of these members of the behavior-pattern, and is clearly evinced in those cases where the closure of subjective want and objective goal is effected by some definite means. The forward direction which investigation takes goes beyond the bare feeling of perception in discerning an attitude of receptivity, an object for which

one is receptive, and a means whereby this object is attained. Yet in the employment of any and all of these means of adjusting the creature more adequately and precisely to its environment, the fundamental motivation of satisfying felt wants is maintained. Originally, one grasps things with hand and mouth. Later one grasps them with eyes and ears. Apprehension, however, is always more than a combination or constellation of phenomena revealed, as it were, upon some inner mirror of the mind, for these phenomena are themselves indications of what is being done at the moment.

The negative side of the pattern of insight is provided by obstacles in the way of attainment and by barriers or repelling forces. When these become dominant the creature's want is overcome and is superseded by the behavior of escape from the field in which the attractive goal is situated. There is then a marked tendency to resort to instinctive behavior without insight and without perception. The creature rushes blindly in the opposite direction, and beats aimlessly against whatever barriers may prevent its escape.

Yet granted the capacities of perception and insight, a barrier to escape in one direction may lead to ingenious devices for escape in other directions. Thus instinctive anger, fear, and disgust are not the only expressions of conflict. Substitutional forms of behavior, in which one does what one can, though not what one would do, offer temporary satisfactions. With abstract as well as concrete tools to work with, one may raise himself theoretically above the field of actuality on to a plane of contemplation where by recourse to ideas and plans one's best hopes are no longer thwarted, and one gains in thought and imagination what one cannot have in reality.

All these substitutional means to ends are derived from actual means to ends, and the behavior of thought and contemplation is itself no less actual than perception. While no longer overt, the behavior is still real; the attitudinal and postural resources of the self have merely acquired gestural, verbal, numerical, and graphic means of representing actions and fulfilments unhindered by the barriers and obstacles of a physical environment.

In order to be able to deal with these inner conflicts and achievements one must have recourse to the experience of them. No purely objective research, such as a biologist is wont to pursue, can ever reveal the minute details of these patterns. They must first be known as they are experienced. Then arises the biological problem of determining the physical status of a process which is so largely completed within the organism.

Even if we knew the physical basis of thinking, it would be absurd to demand an instrumental record that could be read from a scale, in lieu of the direct account that can be so much more readily given by the thinking person. Important as scale-readings may be as direct evidences of what is going on in the brain and other parts of the organism, it will remain the task of the psychologist to set the neurologist's problem for him; because only the psychologist has direct insight into what the organism is doing with itself. This view of behavior from the inside is the special province of psychology, and the problem is no less one of behavior because the indications of it are in a true and unique sense psychological rather than biological or physical in nature.

Cornell University R. M. OGDEN

Attensity, Clearness, and Attention

The greatest obstacle which at present hinders the student of attention is not that of technique. Rather, the dilemma of systematic premise increasingly embarrasses him, for the outstanding fact of the psychology of attention in the last decade is that of a sharper and sharper division into two doctrinal parties.

The first of these schools sees in the phenomenon of attention the manifestation of *sensory* clearness exclusively, in the Titchenerian sense of that adjective. The school enjoys the vigorous leadership of Professor Dallenbach, from whose 1920 paper on "Attributive *vs.* Cognitive Clearness"[1] the whole controversy as an explicit clash of doctrine proceeds; and the subsequent defense of sensory clearness in relation to attention has been conducted for the greatest part by him and his students. Professor Dallenbach's position is consistent with Titchener's and may be epitomized in these terms: the problem of attention is the problem of the pattern of sensory (*i.e.* attributive) clearness as distinguished from all other clearnesses (*e.g.* cognitive clearness). He was thus led in 1920 to reject from the field of attention the classical experiments on range; for "the experiments hitherto made upon 'range of attention' are perceptual experiments, in which the end-result is the range, not of attention, but of cognition," since "in experiments on the range of attention, range has always been determined by the number of objects—lines, letters, words, numbers, figures, etc.,—which can be named or reproduced after a momentary exposure; that is, by the number cognised."[2] And again, in 1926[3] and 1930,[4] he includes in his reviews of work on attention only with reluctance experiments dealing with cognitive clearness.

The second school—embracing those who heterodoxically erect the phenomenon of attention upon *cognitive* clearness—seems to me to be without definite leadership. It is revealed rather by a number of independent investigators who have at one time or another come into conflict with Professor Dallenbach; and it derives unity largely from a common rejection of his doctrine. Professor Dallenbach has himself provided complete references to his critics' papers;[5] they need not be recited here.

It is my opinion that this state of sharp disagreement cannot but retard experimental progress. For the individual investigator, however enthusiastically he may set to work, soon runs against the unhappy necessity of choosing whether to side with a conception of attention which discards with one gesture what has come to be regarded as classical research, or, taking the other side, to risk the demonstration when he is through that he has not, *ex hypothesi,* dealt with attention at all. If one takes the first alternative, one must subscribe to the unattractive persuasion that very nearly all the experiments on attention, as far back as Hamilton, have really contributed nothing to the field. Or, accepting the second, one must perforce take issue with a doctrine that has behind it the considerable prestige of the Titchenerian psychology—the very system that, preëminently, urged the implicative importance of attention. Can either choice quickly tempt the student who recollects the splendidly

---

[1] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.,* 3, 1920, 184-230.
[2] *Ibid.,* 228.
[3] *Idem,* Attention, *Psychol. Bull.,* 23, 1926, 1-18.
[4] *Idem,* Attention, *Psychol. Bull.,* 27, 1930, 497-513.
[5] *Idem,* this JOURNAL, 40, 1928, 337 f. (footnote 3).

confident pronouncement that "the doctrine of attention is the nerve of the whole psychological system?"

And yet we must choose!—else we shall never come to the empirical knowledge of attention that a complete science demands. The quandary is intolerable precisely because the problems that generate it are fundamental.

The burden of this paper is an essay toward such a choice. Its fundamental contention is this: Granted (as will be apparent) that the question whether or not to consider attention as coterminous with attributive clearness is one of sheer definition, and that with respect to definition there is no *a priori* imperative to define one way or another—still, once a definition has been accomplished, its logical and practical consequences must be such as to justify its continued acceptance. And I intend to maintain that the theory of attention in terms of cognitive clearness is superior to that in terms of sensory clearness in those respects.

First, however, we must understand what is meant by attributive (sensory) clearness, or *attensity*, as we shall henceforth call it for simplicity's sake. Our search for definition carries us back to Titchener; and the following quotations are presented as appearing to me to be at once sufficiently concise and representative:

> Clearness [attensity] is an attribute of sensation, which, within certain limits, may be varied independently of the other concurrent attributes.[6]

> Clearness [attensity] . . . is the attribute which gives a sensation its particular place in a consciousness: the clearer sensation is dominant, independent, outstanding, the less clear sensation is subordinate, undistinguished in the background of consciousness.[7]

> We may group all the attributes of sensation under two headings, qualitative and intensive. I should, for instance, rank as intensive attributes, in the broad sense, degree or intensity proper, duration, extension, and clearness [attensity] . . . Clearness . . . varies between a liminal value and the maximum set by the limit of attentional concentration.[8]

> Clearness [attensity], our fourth intensive attribute, is no more definable than its fellows. It is the attribute which distinguishes the 'focal' from the 'marginal' sensation; it is the attribute whose variation reflects the 'distribution of attention.'[9]

We must, of course, amend these definitions in one respect, for, if we are to retain a structuralistic concept of attensity, we must recognize that latter-day structuralism was forced by internal criticism away from the notion of attributes of sensation to the notion of dimensions of sensory experience.[10] But, with the inclusion of this significant change of terminology, a sufficiently faithful characterization of attensity would appear to be something of this sort:

Attensity is an independently variable, intensive dimension of experience, revealed typically in attentional structure, but so primitive as to be otherwise indefinable.

---

[6] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 211.
[7] *Idem, A Text-Book of Psychology*, 1910, 53.
[8] *Idem, Feeling and Attention*, 10.
[9] *Ibid.*, 26.
[10] Cf. Titchener, this JOURNAL, 35, 1924, 156.

As distinct from attensity, we now wish to define or characterize cognitive clearness—or simply *clearness* (as, again, we shall most conveniently call it.) Professor Dallenbach refers us to Titchener for that distinction. The passage is as follows:

When a tachistoscopic field is exposed for the first time to an unpractised observer, he will very probably fail to 'make out' anything at all; the lines or letters or geometrical figures are seen as a general impression, without discrimination of detail. Was, then, the field obscure? Surely not! The observer was 'attending' with all the concentration he could summon; the field was the clearest thing in his consciousness. What he failed to do was to *cognize*. Cognition is not clearness [attensity]; it is an associative process of the assimilative kind. Apperception and cognition are so usually conjoined, in our adult experience, that we may sometimes forget to separate them; but psychologically they are different things. When, then, the practiced observer tells us that some of the details in the exposure-field are 'clear' and others 'half obscure,' he means that he has cognized the former and failed to cognize the latter.[11]

Here again, as with attensity, the experiential characteristic is easier to demonstrate than to define. Nevertheless, certain descriptive terms at once suggest themselves. Cognizability is, naturally enough, the truest synonym of this clearness. And cognizability implies immediately reportability—a point with which Professor Dallenbach no doubt agrees; since, in refutation of Wever, he labels the report of one of the latter's observers to the effect that "a thing is clear in proportion as it can be reported upon" as a characterization of cognitive clearness.[12]

Reportability, in turn, implies adequacy of content in respect of whatever characteristics the observer is, at the moment, interested in reporting; and this is a fact of no little importance. It was deduced originally by Britz, whose research on clearness[13] motivated Professor Dallenbach's 1920 paper. Britz tells us, concerning the experience of his observers when colored rectangles were presented to them tachistoscopically:

A vague patch of color occurs as a conscious content. The observer will naturally say to himself that this color should be considered less perfect than other colors which were simultaneously—but not so vaguely—present. Yet, if he were to see this color under other conditions and isolated, he might well feel no inclination to such a judgment; for neither the opportunity nor the intention to relate one content to others is always at hand. The observer knows that the colors represent rectangles, and this knowledge influences him to rate un-contoured colors lower in clearness than contoured ones; for he is quite convinced that the color *could* appear as a rectangle. Under other circumstances he would hardly ever bring the given content into relation with other possibilities.[14]

And Britz comes to the conclusion that clearness is *"ein Urteilsmässiger Vorgang."*[15]

This notice of the *Urteilsmässigkeit* of clearness is of moment for its assertion that clearness is not only a judgmental affair, but that the judgment varies relative to whatever particular aspects of experience the observer happens to be contemplating. This general observation is, indeed, particularized in Titchener's exam-

[11] *Feeling and Attention,* 238, cited by Dallenbach in this JOURNAL, 40, 1928, 339 (footnote 11).

[12] Dallenbach, Dr. Wever on attention and clearness, this JOURNAL, 40, 1928, 342.

[13] C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit,* 1913.

[14] *Ibid.,* 67; the translation is mine.

[15] *Ibid.,* 75.

ple, given above. There the observer who says the exposure-field is unclear is at once correct and incorrect: correct, because if he expects to be able to see well-defined letters or lines, his experience is inadequate to that expectation, and hence unclear; and incorrect, because, relative to a possible expectation of "a general impression without discrimination of detail," his experience is in truth adequate and clear. We may, with Britz and Professor Dallenbach, regard the various degrees of adequacy of detail with which, *e.g.* a visual impression may occur—from a mere localizable blotch to a determinately bounded and colored figure—as levels of cognition. A given impression is clear, then, relative to the level of cognition to which its experienced properties are adequate, and less clear relative to any higher level. And so, before we can make a judgment as to clearness, we must presuppose an expectation or an *Einstellung*. Adequacy for report-from-such-*Einstellung* constitutes, in consequence, the measure of clearness.

In fine then, we may characterize clearness (in our sense of cognitive clearness) *as determinateness or adequacy of experience in respect of agreed-upon properties, and measurable by the reportability* (for example) *of such properties.*

With the natures of attensity and (cognitive) clearness, respectively, now in mind, I wish to offer my objections to a doctrine that bases the attentional process on the first of these affairs exclusively.

The first criticism which comes to mind rests upon the evident disparity between an attention of attensity and the large pre-psychological problem of attention which scientific psychology adopted. Attention—even restrictedly as a mode of structure of experience—is no recondite or academic phenomenon. Its observation lies close at hand and is accomplished frequently and independently of instruction by the naïvest individuals. Despite the popular confinement of the term *attention* to what we should call *secondary attention,* the student who is introduced in class to the law of the selectiveness of conscious representation as a sheer general fact recognizes in that fact something strongly familiar, from the recollection of numberless instances. He knows already that he can 'pay attention' to one thing only at the cost of the vididness of other simultaneous experiences; if he attends to one of a number of persons talking in his presence, the others begin immediately to utter mere nonsense sounds; and where his interest is absorbed by one event, his impression of concurrent happenings (which he is astonished not to have noticed) is often inadequate both at the moment and for recall. We may agree with James that "everyone knows what attention is. It is the taking possession by the mind, in clear and vivid form, of one out of what seem several simultaneously possible objects or trains of thought. Focalization, concentration, of consciousness are of its essence. It implies withdrawal from some things in order to deal effectively with others."[16]

There is inherent in pre-psychological descriptions of attention the variously expressed notion of equivalence between high degree of cognitive clearness and focality for experience. The allusion to it by way of James' statement of effective dealing with parts of consciousness is patent; the common remark—"I didn't pay enough attention to understand it"—reveals the same thought; and Titchener's untrained observer, who falls so easily into judgments of clearness rather than attensity, thus gives expression to his untutored belief.

[16] W. James, *The Principles of Psychology,* 1890, I, 403 f.

Our natural expectation would, therefore, be to find the psychologist subsuming the phenomenon of attention under scientifically derived laws of cognitive clearness. The layman (and conceivably a number of psychologists!) would be, presumably, well satisfied with the statement that attention is a matter of clearness. Curiously, however, the structuralists have resisted the idea.

The resistance, as we have said, is evidently not predictable from the mere common-sense of attention. It ought therefore to have behind it some very solid argument of a high level of sophistication. But to look for this argument in the precise place where one would expect to find it is to invite disappointment. Recall, for a moment, how the marriage of attention and attensity is carried off in *Feeling and Attention:* "It seems to me beyond question," Titchener writes, "that the problem of attention centres in the fact of sensible clearness. Let me call my witnesses. . . ."[17] There follow five pages of quotation from Wundt, Pillsbury, Ribot, Ladd, Stumpf, Stout, Baldwin, Bradley, and Ferrier. All are agreed that attention is characterized by the clearness of certain contents, or at least by a condition reasonably synonymous with clearness; so that Titchener is quite right in his statement that "wherever you look, you find some form of reference to clearness." But none of these witnesses has the slightest contribution to make toward an identification of clearness with *attensity*—least of all, Wundt! So that, when Titchener concludes—"I have been obliged to turn up a small library of references, in order to make sure that my position is well taken. With that assurance gained, let us proceed to a study of clearness as an attribute of sensation"[18]—we may well take exception to this gratuitous feeling of security.

Beyond its *non-sequitur* from independent authority, the Titchenerian doctrine is furthermore essentially circular in logic. On the one hand, we have the definition of attensity as "the attribute whose variation reflects the distribution of attention," and subsequently we come upon the idea that "attention centres in the fact of sensible clearness." The one definition seems always to imply and employ the other, in whatever order one proceeds. Titchener's answer to this is that "you cannot say much about a thing you regard as ultimate to your science."[19] And if attensity, or attention, are both or either ultimate, they may thus forever escape definition. But the fact remains that, for this very reason, the definition of attention in terms of attensity can never of itself compel acceptance.

If attensity is really indefinable and only demonstrable, perhaps the argument from attensity to attention would be apparent from Titchener's examples of the phenomenon. But, with respect to many such illustrations, the logical sequence is no better. For instance, no one can have read the exposition of attention in the *Textbook* without recalling the space there devoted to the picture puzzle as evidence of the shift of attention. The particular puzzle is a drawing of the human brain, the convolutions of which, on inspection, turn out to be cleverly designed little babies.

Look at it, and try to find what it conceals. While you are searching, the whole drawing is at the upper level of consciousness, and the rest of your experience is at the lower level. Suddenly you find what you are looking for: and what happens? Why the moment you do so, the picture of the brain drops clean away from the

[17] *Feeling and Attention,* 182.
[18] *Ibid.,* 188.
[19] Titchener, The psychological concept of clearness, *Psychol. Rev.,* 24, 1917, 55.

upper level: the concealed outlines stand out with all imaginable clearness, and the form of the brain is no clearer than the feel of the book in your hand.[20]

The Gestalt-theorists have already dealt adversely with the hypothesis that such shifts in equivocal perceptions are shifts of attensity.[21] Their criticism rests on the following very reasonable point. In the case of the brain-drawing, for example, the original perception is not one of clear brain-form and unclear baby-form. The baby-form is simply not there to begin with, and hence it cannot be assigned any degree of attensity except zero—which is synonymous with non-existence. The assertion that "concealed" forms are usually really non-existent is supported empirically by evidence showing that when they are no longer "concealed," there is a distinct break for recognition in the identity of the stimulus.[22] Unless the structuralist can reply with contrary experimental evidence, the configurational argument removes equivocal perceptions from the domain of attention-*qua*-attensity.

Or, again, Titchener treated of range of attention in the *Text-book*. We measure range, he tells us, by presenting visually, *e.g.* "a number of dots, lines, letters, numerals, simple geometrical figures, bands of colors, *etc.*"[23] But Professor Dallenbach has already pointed out the inconsistency between the measurement of attention where such stimuli as these must be cognized, and the non-cognitive nature of attensity. Thus it is that, although we owe the formulation of the theory of attention and attensity to Titchener, his own treatment of the subject varies extraordinarily between what is consistent with that doctrine and what is the proper study of cognitive clearness.

I have criticized the Titchenerian dogma on the scores of its disregard of the pre-psychological formulation of attention and of independent psychological authority, of its internal logical circularity, and of its inconsistence with purported examples and measurement of attention. Let us now occupy ourselves with an examination of the fruits of this doctrine.

On the side of experimental productivity, the consequences have been negligible. More than twenty years of opportunity for research since the firm establishment of the doctrine have produced almost no new knowledge about attention-*qua*-attensity beyond the laws originally stated by Titchener. The exceptions seem to me to be only three in number.[24] The first is the evidence of Geissler[25] for the multiple-level structure of the attentive consciousness (in, perhaps, the case of only certain observers); the second is Professor Dallenbach's contrary evidence for a dual-level structure[26] (again, perhaps, for certain observers); and the third is Professor Dallenbach's evidence for extending the constant of the range of attention to a much larger

---

[20] *Text-book*, 277.

[21] This objection has been reëmphasized lately by H. Helson, Studies in the theory of perception. I. The clearness-context theory, *Psychol. Rev.*, 39, 1932, 44-72.

[22] Cf. W. Köhler, *Gestalt Psychology*, 1929, 205-215, and E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 28.

[23] *Text-book*, 287.

[24] I wish to stress my belief that these are the only important studies whose contributions to the theory of attention-*qua*-attensity might not equally well be regarded as contributions to a theory of attention-*qua*-cognitive-clearness.

[25] L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 524ff.

[26] K. M. Dallenbach, Attributive *vs.* cognitive clearness (note 1 *supra*).

number than the traditional six.[27] And, as respects the security of these three demonstrations, the first and second are obviously in conflict, and the third has been shown by Fernberger (as we shall later observe) to involve a method of measurement which defeats its own ends. It would be distinctly unfair not to mention the series of *Cornell Studies on Conditions of Attention*[28] now in progress; but these are rather more supplements and refinements of Titchener's original laws than demonstrations of new principles.

Or, again, the theoretical consequences of the Titchenerian doctrine have lagged. The forceful utterance of a doctrine in psychology usually prepares the way for a series of attempts to correlate the variables of that theory with physiological variables. Of the original five dimensions of experience, considerable progress has been made in the tying down of four to cerebral and recepto-neural events.[29] And, if our knowledge of the physiology of intensity, quality, extensity, and duration is still defective, the lacunae are at least subject to competent and enthusiastic speculation. But attensity alone of the equally primitive dimensions seems not to have aroused the discussion or creation of physiological hypotheses that its alleged importance would warrant. The student who inquires after the possible cerebral basis of attensity may quite safely be sent back to Titchener to find that attensity has, perhaps, something to do with "reënforcement" and "inhibition."[30] He will not be able to discover why the results of reënforcement and inhibition are *at*tensive changes, and not, as would seem more likely, *in*tensive changes. But he will at least have consulted the latest word on the matter!

A particularly unhappy development is the apparent impasse which has been reached in the measurement of the range of attention. Strict Titchenerian tenets required, of course, that the older methods involving calculations of the number of objects which could be cognized should be discarded; and it was necessary, therefore, to devise some method whereby it could be determined how many conscious contents can simultaneously share high attensity, irrespective of their clearness. Gill and Dallenbach[31] selected for this purpose the method of constant stimuli. Groups of simple stimuli of various numbers were presented to the observer, and he was required to report whether all, or only some, of the resulting impressions were at the upper level of attensity; from the data the upper limen (in terms of number of stimuli) for the uni-level effect was calculated, and employed as a measure of the range of attention.

---

[27] N. F. Gill and Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 247-256; A. D. Glanville and Dallenbach, The range of attention, this JOURNAL, 41, 1929, 207-236.

[28] A complete list of these Studies will be found in A. M. White and Dallenbach, this JOURNAL, 44, 1932, 175.

[29] Summaries of this progress have been provided most recently by E. G. Boring, The physiology of consciousness, *Science*, 75, 1932, 32-39, and L. T. Troland, *The Principles of Psychophysiology*, 1931-32, especially vols. I (Sensation) and III (Cerebration). It seems to me very much to the point that Troland has, furthermore, a good deal of much interest to say about the possible physiology of cognitive clearness, around which his discussion of attention is built (Vol. III, 137-154), whereas he has not similarly speculated on the physiology of Titchenerian attensity.

[30] The most explicit of Titchener's treatments of the physiological basis of clearness and attention is in his article, Attention, *New International Encyclopedia*, 2nd ed., 1923, II, 342.

[31] *Op. cit.*

Ingenious as this method is, it failed of its purpose. For, as Fernberger has pointed out, what we are here measuring is simply the number of stimuli which statistically is adequate to a uni-level impression, and not the number of *conscious contents* which can concurrently occupy the upper level—whereas the latter measure is the one with which questions of range are concerned. "What assurance," Fernberger asks, "did Gill and Dallenbach have that the *Os saw* all of the elements on a given card? This is apparently assumed. It has been our experience that, with exposures as rapid as 60 sigma, many elements may be on a card which are not seen at all. My own and Oberly's findings indicate that this happens when stimulus cards contain only 3 or 4 elements."[32]

The essence of Professor Dallenbach's reply to this criticism is: "Truly we have no such assurance, and what is more to the point we did not care whether our *O*s saw all the elements, half of them, or only a small fraction of them. All the elements had the same opportunity of being seen; beyond that our interest did not go. The question whether all the items were seen no more concerned us than the question whether all the grams in a 100-grm. weight were sensed would concern Fernberger in a lifted-weight experiment. The method of constant stimuli, as Fernberger knows, is adequate to such subjective variables. What we were solely interested in was to discover, by the method of constant stimuli, the number of dots that would correlate 50% of the time with one level of clearness."[33]

This reply seems clearly unsatisfactory; the analogy suggested is hardly proper, and the whole statement does not make of the experiment the measure of range which Fernberger demands. The most unfortunate aspect of the matter is not that this particular experiment failed to measure the range of attention-*qua*-attensity, but that it demonstrates the strong likelihood (also noted by Fernberger) that such a measure can *never* be obtained by the known psychophysical methods. The reason for this is that in order to determine the answer to the question of how many conscious items may occupy the upper level, the observer must count or otherwise identify the items—and hence the measure will involve cognitive clearness. Thus the very mechanism of measurement substitutes clearness for attensity.

In fact, this point is capable of significant extension beyond the particular case of a quantitative psychophysical experiment. *Any sort of report whatsoever on attensity is demonstrably subject to the limitations of cognitive clearness.* For there is ultimately no difference in this respect between a report which says "this item of experience is circular, or square, or a letter of the alphabet" and a report which says "this item of experience is attense." The first report involves cognition, but so does the second—the cognition of attensity. Whoever would maintain that, for the purposes of psychology, certain aspects of experience are handled free of cognitive interpretation is at variance with the—almost painfully learned—consensus of contemporary science. And attensity is naturally no exception. It may have been thought an exception in the hey-day of the sensation; but with the realization that, as Koffka puts it,[34] the sensation is not prior to meaningful organization, but actually

[32] S. W. Fernberger, The 'range of attention' experiment, this JOURNAL, 38, 1927, 478.

[33] K. M. Dallenbach, Dr. Fernberger on the 'range of attention' experiment, this JOURNAL, 38, 1927, 481.

[34] K. Koffka, Zur Theorie der Erlebnis-Wahrnehmung, *Annal. d. Philos.*, 3, 1923, 396: "Dann ist, in weiterem Sinn, die Analyse auch ein Gestaltungsvorgang. Denn

a product of one such organization, not even the attributes of sensation can escape cognitive interpretations. Professor Dallenbach regards attensity as one of the conditions of (cognitive) clearness.[35] That is very likely; but we now need to add that for practical purposes (that is to say, for the purposes of empirical psychology), clearness is a necessary condition of the reportable knowledge of attensity. And, consequently, he who would discuss attensity with no thought to cognition may well take pause.

Another difficulty at which the doctrine of attention-in-terms-of-attensity has arrived is that of sharp divergences in the interpretation of published experimental results; the peculiar idefinability of attensity has not made it a happy topic of discussion. An example will suffice: namely, the controversy between Professor Dallenbach and Wever over the latter's experiment on the figure-ground perception. Of this controversy the salient point is Professor Dallenbach's conviction that Wever "does not know his clearnesses"[36] because his protocols are inadequate to a demonstration of the difference between cognitive and attributive clearness. The accusation might also be levelled against Wever's observers with equal justification. The inescapable conclusion is that some observers can reliably observe attensity and others can not. Professor Dallenbach's observers seem to fall into the first class and Wever's into the second; although it must be pointed out that even Professor Dallenbach's observers in 1920 failed to report attensity in almost one-fifth of their observations, despite instructions.[37] A phenomenon which varies thus at the hands of observers of different laboratories makes inevitably for anything but scientific accord and security. One cannot read without a certain sympathy Crosland's recent lament[38] over the possibilities for almost personal misunderstanding which lie very near the surface of this field of discussion.

Finally, it may be asked of the doctrine of attention and attensity, whether it has in the years since its conception shown any really essential relevance to the rest of psychology—such relevance, for instance, as would justify Titchener's notion of its fundamental importance. The answer here is strongly in the negative. We can demonstrate this truth by considering the present status of two fields of psychology in which, according to Titchener, attention was of large importance. The first field is that of psychological analysis; and here we have already remarked that the products of analysis are now generally considered to be particular meaningful *Gestaltungen*, and, therefore, in a strong sense, akin to the phenomenon of equivocal perception, in that they represent restructurings in the presence of constant stimuli. Here is no indication of the rôle of attensity, any more than in the case of the puzzle-picture. Here is rather a fact of structure and cognition, and, in fine, of clearness as we have defined it—the adequacy of experience to preconceived categories of report. The

---

indem wir die Teile aus dem Ganzen Lösen, bekommen sie nun eine eigene Gestalt. Unter diesem Gesichtspunkt ist also die Empfindung nicht etwas dem Gestaltungsprozess vorhergehendes, sondern ein höchstes Erzeugnis einer besonderen Gestaltung."

[35] Dallenbach, this JOURNAL, 40, 1928, 341.

[36] *Ibid.*, 340.

[37] Dallenbach, *op. cit.*, 1920, 200, Table IV. If one excludes the experimenter's own observations from this table, retaining only the two *O*s who worked without knowledge of the problem, the percentage of reports which omit mention of attensity rises to 23.7.

[38] H. R. Crosland, Letter-position effects in the range of attention experiment, etc., *J. Exper. Psychol.*, 14, 1931, footnote 17 on pp. 501ff.

second field is that of memory. Focality in attention was, in Titchenerian psychology, an important condition for association. And it still is, if attention be defined in terms of clearness; for that which is cognized as a unit persists as a unit, to which the configurational work on memory bears testimony. Translate 'focality in attention' into 'attenseness,' and where is the evidence for such a law, devoid of cognitive implications? In these two fields the significant relation is to clearness in the cognitive sense. And yet the fields are those in which the Titchenerian doctrine of attention promised great implication.

Our criticism of the attensive theory of attention has now extended over a number of considerations. The positive contribution of the discussion issues from it quite simply. My suggestion is this: that *the study of attention would profit largely if we were to state the phenomenon of attention in terms of cognitive clearness.*

I venture to point out that a doctrine of attention, formulated in this way, would not only receive justification from the nature of the general pre-scientific problem to which it would correspond, but it would avoid the very same difficulties—logical and experimental—that we have seen embarrassing the Titchenerian dogma. More than that, it would embrace the results of much important work that at present is rigidly excluded from the attentional field or whose implications are obscured. We should see, for instance, that the bond between the abstraction-experiment and the theory of attention is of the closest; for the results on abstraction would be sensed as showing the dependence of clearness upon particular *Aufgaben*. We should see, again, that in Spearman's suggestion, for example, of a "constancy of cognitive output"[39] there is a broad hint toward a theory of range. And—what is of no little desirability—we should be able to avoid the confusing and superfluous trichotomizing of the problem of range into ranges of "attention," "cognition," and "apprehension,"[40]—a process which imposes an unnecessary and idle complication of terminology upon what deserves to be an orderly, intact field of investigation.

What I am suggesting is a retrogression—but a retrogression in definition only, not in knowledge. Its goal lies very near to what we can conceive the original problem of attention to have been. The psychologist may naturally be cautious in accepting the counsel of popular introspection. But granting him that caution, there is yet a wealth of historical evidence (as in the present instance) to demonstrate that a provocative problem inherited from lay observation may turn sterile under too artificial cultivation. Does it not appear more than likely that a reformulation of the theory of attention in terms of cognitive clearness would unify a fragmentary field of psychology and endow it with fresh implication?

Harvard University DWIGHT W. CHAPMAN

PORTRAITS USEFUL TO THE PSYCHOLOGIST

The following catholic list of portraits is published for the convenience of teachers and lecturers who may wish, particularly when dealing with the history of psychology or with the contemporary schools, to show pictures of the men who have founded schools, have led movements, or have taken a prominent part in the estab-

[39] C. Spearman, *The Nature of 'Intelligence' and the Principles of Cognition*, 1923, 131.

[40] H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-353; Further results on the range for visual attention, cognition, and apprehension, *ibid.*, 37, 1926, 132-138.

lishment or furtherance of a point of view. The portraits, besides showing the manner of the man with whom the speaker is dealing, serve to lighten an exposition which is apt to be heavy, and to give visual as well as auditory cues to the principal actors in the story. These advantages, however, are frequently lost because of the difficulty experienced in obtaining pictures of the men desired. Though photographs have been published of most of the important men in psychology, it often requires long and patient search to find the picture of a given individual. This difficulty will, we believe, be reduced to a great extent by the present list.

All the portraits found in the books and periodicals of the Psychological Library of Columbia University are given here. Different pictures of the same individual are listed in chronological order and are numbered consecutively; copies are listed after the original without numbers.[1] For example; only three of the five portraits of Alfred Adler are different. These three are numbered and listed chronologically. The second (2) of these was reproduced in two other books. These reproductions are given without numbers and in chronological order after (2)—the original.

ABRAHAM, KARL (1876-1925). *Int. Zsch. f. Psychoanal.*, 12, 1926, frontispiece no. 2.

ADLER, ALFRED (1870-——). (1) G. S. Hall, *Life and Confessions*, 1923, 217. (2) *Wittenberg Symposium*, 1928, 3; W. B. Pillsbury, *The History of Psychology*, 1929, facing 253; *Psychologies of 1930*, xiv. (3) *Int. Zsch. f. indiv. Psychol.*, 8, 1930, frontispiece (with signature).

ANGELL, JAMES ROWLAND (1869-——). *Scient. Mo.*, 29, 1929, 380.

ARPS, GEORGE FREDERICK (1874-——). *Encycl. Psychol.*, 1928, 38.

AVELING, FRANCIS (1875-——). *Wittenberg Symposium*, 1928, 3.

BAIN, ALEXANDER (1818-1903). J. Sully, *My Life and Friends*, 1918, facing 86.

BALDWIN, JAMES MARK (1861-——). *A History of Psychology in Autobiography*, I, 1930, xiv.

BECHER, ERICH (1882-1929). *Arch. f. d. ges. Psychol.*, 68, 1929, frontispiece (with signature).

BEKHTEREV, VLADIMIR M. (1857-1927). (1) *Festschrift für Bekhterev*, 1926, frontispiece (with signature). (2) L. F. Grote, *Die Medizin im Selbstdarstellungen*, 1927, facing 1 (with signature); *Wittenberg Symposium*, 1928, 3; *Quar. Rev. Biol.*, 3, 1928, facing 505. (3) *Scient. Mo.*, 26, 1928, 444.

BELL, CHARLES (1774-1843). F. Fearing, *Reflex Action*, 1930, facing 112.

BENTLEY, MADISON (1870-——). (1) *Psychologies of 1925*, facing 383; *Ped. Sem.* 33, 1926, following 152. (2) *Wittenberg Symposium*, 1928, 3. (3) *Psychologies of 1930*, xiv.

BIANCHI, LEONARDO (1848-1927). *Ment. Hygiene*, 14, 1930, suppl. following 462.

BINET, ALFRED (1857-1911). (1) *Année psychol.*, 18, 1911, frontispiece (with signature); *Ment. Hygiene*, 14, 1930, suppl. following 462. (2) *Anné psychol.*, 18, 1911, facing 15 (with signature). (3) H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 4.

BLONDEL, CHARLES AIMÉ ALFRED (1876-——), *Psyche*, 6, 1926, frontispiece no. 24.

BORING, EDWIN GARRIGUES (1886-——). (1) *Psychologies of 1930*, xiv. (2) *Scient. Mo.*, 32, 1931, 90.

BOURDON, BENJAMIN BIENAIMÉ (1860-——). *History of Psychology in Autobiography*, II, 1932, xiv.

BRAID, JAMES (1795-1860). *Scient. Mo.*, 29, 1929, 157.

BRENTANO, FRANZ (1838-1917). *Amer. J. Psychol.*, 36, 1925, facing 469 (with signature).

[1] The reproductions, even those taken from copyrighted books and magazines, are not always acknowledged. Some, according to statements made in preface, introduction, or text, are reprinted with permission, but a surprisingly large number seem to have been pirated.

BRETT, GEORGE SIDNEY (1869——). (1) *Wittenberg Symposium,* 1928, 4; *Psychologies of 1930,* xiv.

BREUER, JOSEPH (1842-1925). G. S. Hall, *Life and Confessions,* 1923, 217.

BRODMANN, KORBINIAN (1868-1918). *J. f. Psychol. u. Neurol.,* 24, 1919, facing i (with signature).

BÜHLER, KARL (1879——). *Wittenberg Symposium,* 1928, 4.

CAJAL, SANTIAGO RAMÓN Y (1852——). (1) *Clark Univ. Decentennial Celebration,* 1899, facing 311 (with signature); *Mental Hygiene,* 14, 1930, suppl. following 462. (2) *Scient. Mo.,* 31, 1930, 179.

CALKINS, MARY WHITON (1863-1930). *A History of Psychology in Autobiography,* I, 1930, xiv.

CANNON, WALTER BRADFORD (1871——). *Wittenberg Symposium,* 1928, 4.

CARR, HARVEY A. (1873——). (1) *Wittenberg Symposium,* 1928, 4; *Quart. Rev. Biol.,* 3, 1928, facing 505. (2) *Psychologies of 1930,* xv.

CATTELL, JAMES MCKEEN (1860——). (1) *Wittenberg Symposium,* 1928, 12; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 198. (2) *Scient. Mo.,* 28, 1929, 25.

CHARCOT, JEAN MARTIN (1825-1893). (1) G. S. Hall, *Life and Confessions,* 1923, 216; *Scient. Mo.,* 29, 1929, 159. (2) *Ment. Hygiene,* 14, 1930, suppl. following 462.

CLAPARÈDE, EDOUARD (1873——). (1) *Wittenberg Symposium,* 1928, 5. (2) *A History of Psychology in Autobiography,* I, 1930, xiv.

COUÉ, EMILE (1857-1926). *Scient. Mo.,* 29, 1929, 159.

DEWEY, JOHN (1859——). *Psychologies of 1930,* xv.

DE SANCTIS, SANTE (1863——). *Psyche,* 6, 1926, facing 56, no. 21.

DODGE, RAYMOND (1871——). *A History of Psychology in Autobiography,* I, 1930, xiv.

DREVER, JAMES (1873——). *A History of Psychology in Autobiography,* II, 1932, xiv.

DUNLAP, KNIGHT (1875——). (1) *Psychologies of 1925,* facing 309; *Ped. Sem.,* 32, 1925, following 386; *Wittenberg Symposium,* 1928, 5. (2) *Psychologies of 1930,* xv; *A History of Psychology in Autobiography,* II, 1932, xiv.

EBBINGHAUS, HERMANN (1850-1909). (1) *Zsch. f. Psychol.,* 51, 1909, frontispiece (with signature); H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 50; *Amer. J. Psychol.,* 42, 1930, facing 505 (with signature).

FECHNER, GUSTAV THEODOR (1801-1887). (1) G. S. Hall, *Founders of Modern Psychology,* 1912, facing 125; H. E. Garrett, *Great Experiments in Psychology,* 1930, facing 270.

FERNALD, WALTER ELMORE (1859-1924). (1) *Ment. Hygiene,* 9, 1925, frontispiece; *J. Psycho-Asthenics,* 30, 1925, facing 16; *Ment. Hygiene,* 14, 1930, suppl. following 462; S. P. Davies, *Social Control of the Mentally Deficient,* 1930, facing 24.

FERRARI, GIULIO CESARE (1868——). (1) *Ment. Hygiene,* 14, 1930, suppl. following 462; *A History of Psychology in Autobiography,* II, 1932, xiv.

FERRIER, DAVID (1843-1928). *J. Ment. Sci.,* 74, 1928, facing 375.

FLUGEL, JOHN CARL (1884——). *Psychologies of 1930,* xv.

FOREL, AUGUST (1848——). (1) *Clark Univ. Decentennial Celebration,* 1899, facing 409 (with signature). (2) *J. f. Psychol. u. Neurol.,* 13, 1908, frontispiece (with signature). (3) L. R. Grote, *Die Medizin im Selbstdarstellungen,* 1927, (with signature). (4) *Psyche,* 8, 1928, frontispiece no. 32.

FRANZ, SHEPHERD IVORY (1874——). *A History of Psychology in Autobiography,* II, 1932, xv.

FREUD, SIGMUND (1856——). (1) G. S. Hall, *Life and Confessions,* 1923, 335. (2) F. Wittels, *Sigmund Freud,* 1924, frontispiece (with signature). (3) *Int. Zsch. f. Psychoanal.,* 12, 1926, frontispiece no. 3. (4) *Psyche,* 6, 1926, frontispiece no. 23; W. B. Pillsbury, *The History of Psychology,* 1929, facing 253. (5) *Ment. Hygiene,* 14, 1930, suppl. following 462.

GALTON, FRANCIS (1822-1911). (1-48) K. Pearson, *Life, Letters and Labours of Francis Galton,* I, 1914; II, 1924; IIIA, IIIb, 1930 (numerous portraits, and

several silhouettes and sketches made at ages of 8-88 years; some reproduced with signature). (49) *Scient. Mo.*, 30, 1930, 124; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 170.

GAULT, ROBERT HARVEY (1874——). *Wittenberg Symposium*, 1928, 5.

GOLGI, CAMILLO (1843-1926). *J. Ment. Sci.*, 72, 1926, facing 453.

GÖTZ, MARTIUS (1853-1928). *Arch. f. d. ges. Psychol.*, 61, 1928, facing 273 (with signature).

GROOS, KARL (1861——). *A History of Psychology in Autobiography*, II, 1932, xv.

HALL, MARSHALL (1790-1857). F. Fearing, *Reflex Action*, 1930, facing 123.

HALL, G(RANVILLE) STANLEY (1844-1924). (1) *Amer. J. Psychol.*, 14, 1903, facing 267 (with signature). (2) G. S. Hall, *Life and Confessions*, 1923, frontispiece (four pictures at ages of 6, 14, 22 and 35 years). (3) *Ibid.*, 50 (four pictures at ages of 6, 14, 22 and 35 years). (4) *Ibid.*, 99 (age 45). (5) *Ibid.*, 335 (with group of psychoanalysts). (6) *Amer. J. Psychol.*, 35, 1924, facing 313 (with signature); L. Pruette, *G. Stanley Hall*, 1926, frontispiece. (7) *Scient. Mo.*, 30, 1930, 116.

HARTMANN, EDUARD VON (1842-1906). G. S. Hall, *Founders of Modern Psychology*, 1912, facing 181.

HELMHOLTZ, HERMANN L. F. VON (1821-1894). (1) G. S. Hall, *Founders of Modern Psychology*, 1912, facing 247; *Life and Confessions*, 1923, 217. (2) *Helmholtz's Treatise on Physiological Optics*, Eng. trans., I, 1924, frontispiece (with signature).

HERVIEU, PAUL (1857-1915). *Année psychol.*, 10, 1904, frontispiece.

HEYMANS, GERARDUS (1857-1930). *A History of Psychology in Autobiography*, II, 1932, xv.

HOBHOUSE, LEONARD TRELAWNEY (1864-1929). *Quar. Rev. Biol.*, 3, 1928, facing 502.

HØFFDING, HARALD (1843-1931). (1) *A History of Psychology in Autobiography*, II, 1932, xv. (2) *Harold Høffding in Memoriam*, 1932, frontispiece (with signature). (3) *Arch. f. d. ges. Psychol.*, 86, 1932, facing 289 (with signature).

HOISINGTON, LOUIS BENJAMIN (1883——). *Wittenberg Symposium*, 1928, 5.

HOWARD, DELTON THOMAS (1883——). *Wittenberg Symposium*, 1928, 6.

HUNTER, WALTER S. (1888——). (1) *Psychologies of 1925*, facing 83; *Ped Sem.*, 33, 1926, following 152. (2) *Psychologies of 1930*, xvi.

JAENSCH, ERICH (1883——). *Wittenberg Symposium*, 1928, 6.

JAMES, WILLIAM (1842-1910). (1) *Essays: Philosophical and Psychological in Honor of Wm. James*, 1908, frontispiece. (2) J. Sully, *My Life and Friends*, 1918, facing 316. (3) H. James, *The Letters of Wm. James*, 1920, I, frontispiece (age 65). (4) *Ibid.*, 20 (age 18). (5) *Ibid.*, 86 (age 25). (6) *Ibid.*, II, frontispiece (age 53). (7) *Ibid.*, facing 134 (with Josiah Royce). (8) *Ibid.*, facing 160 (with Henry James). (9) *Ibid.*, facing 314 (with Henry Clement). (10) *Scient. Mo.*, 12, 1921, 382; 30, 1930, 114; R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, frontispiece. (11) G. S. Hall, *Life and Confessions*, 1923, facing 183. (12) R. C. Givler, *Psychology*, 1922, facing 252; W. B. Pillsbury, *The History of Psychology*, 1929, frontispiece. (13) M. Münsterberg, *Hugo Münsterberg*, 1922, facing 132.

JANET, PIERRE (1859——). (1) *Psyche*, 7, 1926, frontispiece no. 26; *Wittenberg Symposium*, 1928, 6; *Psychologies of 1930*, xvi. (2) *A History of Psychology in Autobiography*, I, 1930, xv.

JASTROW, JOSEPH (1863——). (1) *Wittenberg Symposium*, 1928, 6. (2) *A History of Psychology in Autobiography*, I, 1930, xv.

JENNINGS, HERBERT SPENCER (1868——). *Quar. Rev. Biol.*, 3, 1928, facing 502.

JONES, (ALFRED) ERNEST (? ——). *Int. Zsch. f. Psychoanal.*, 15, 1929.

JØRGENSEN, CARL AAGE VALDEMAR (1888——). *Wittenberg Symposium*, 1928, 7.

JUDD, CHARLES HUBBARD (1873——). *A History of Psychology in Autobiography*, II, 1932, xvi.

KATZ, DAVID (1884——). *Wittenberg Symposium*, 1928, 7.

KELLEY, TRUMAN LEE (1884——). (1) *Scient. Mo.*, 28, 1929, 39; *Psychologies of 1930*, xvi.

KIESOW, FRIEDRICH (1858——). (1) *Arch. f. d. ges. Psychol.*, 65, 1928, frontispiece (with signature); *Wittenberg Symposium*, 1928, 7. (2) *A History of Psychology in Autobiography*, 1, 1930, xv.

KLINE, LINUS WARD (1866——). *Quar. Rev. Biol.*, 3, 1928, facing 502.

KOFFKA, KURT (1888——). (1) *Psychologies of 1925*, facing 129; *Ped. Sem.*, 32, 1925, following 566; *Psyche*, 8, 1927, frontispiece no. 30; *Psychologies of 1930*, xvi.

KÖHLER, WOLFGANG (1887——). (1) *Psychologies of 1925*, facing 145; *Ped. Sem.*, 32, 1925, following 566; W. B. Pillsbury, *The History of Psychology*, 1929, facing 253; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 248. (2) *Psychologies of 1930*, xvii.

KORNILOV, KONSTANTIN NIKOLAEVITCH (1879——). *Psychologies of 1930*, xvii.

KÖNIG, ARTHUR (1856-1901). *Zsch. f. Psychol.*, 27, 1902, facing 145 (with signature).

KRAEPELIN, EMIL (1856-1926). (1) *Arch. f. d. ges. Psychol.*, 58, 1927, frontispiece (with signature); *J. Ment. Sci.*, 73, 1927, facing 361. (2) *Psychol. Arbeit.*, 9 1927, follows 358 (portrait of sculptured bust). (3) *Ment. Hygiene*, 14, 1930, suppl. following 462.

KRUEGER, FELIX EMIL (1874——). *Wittenberg Symposium*, 1928, 7.

LADD, GEORGE TRUMBULL (1842-1921). *Scient. Mo.*, 30, 1930, 117.

LADD-FRANKLIN, CHRISTINE (1874-1930). *Scient. Mo.*, 30, 1930, 377.

LANGFELD, HERBERT SIDNEY (1871——). (1) *Wittenberg Symposium*, 1928, 8. *Scient. Mo.*, 34, 1932, 88.

LASHLEY, KARL SPENCER (1890——). *Scient. Mo.*, 30, 1930, 571.

LEHMANN, ALFRED (1858-1921). *Arch. f. d. ges. Psychol.*, 42, 1922, frontispiece.

LEWES, GEORGE HENRY (1817-1878). J. Sully, *My Life and Friends*, 1918, facing 262.

LIPMANN, OTTO (1880——) *Zsch. f. angew. Psychol.*, 36, 1930, frontispiece (with signature).

LOEB, JACQUES (1859-1924). *Quar. Rev. Biol.*, 3, 1928, facing 488.

LOMBROSO, CESARE (1836-1909). *Ment. Hygiene*, 14, 1930, suppl. following 462.

LOTZE, RUDOLPH HERMANN (1817-1881). G. S. Hall, *Founders of Modern Psychology*, 1912, facing 65.

LUBBOCK, JOHN (1834-1913). *Quar. Rev. Biol.*, 3, 1928, facing 488.

LUDWIG, KARL (1816-1895). G. S. Hall, *Life and Confessions*, 1923, 217.

MAUDSLEY, HENRY (1835-1918). (1) *J. Ment. Sci.*, 64, 1918, facing 117; *Ment. Hygiene*, 14, 1930, suppl. following 462.

MCDOUGALL, WILLIAM (1871——). (1) *Psychologies of 1925*, facing 273; *Ped. Sem.*, 33, 1926, frontispiece. (2) *Wittenberg Symposium*, 1928, 8; *Psychologies of 1930*, xvii. (3) *A History of Psychology in Autobiography*, I, 1930, xv.

MESMER, FREDERICK ANTON (1733-1815). *Scient. Mo.*, 29, 1929, 155.

MEUMANN, ERNST (1862-1915). (1) *Arch. f. d. ges. Psychol.*, 34, 1915, following 156. (2) *J. Educ. Psychol.* 6, 1915, 427.

MEYER, ADOLF (1866——). *Ment. Hygiene*, 14, 1930, suppl. following 462.

MORGAN, C(ONWY) LLOYD (1852——). (1) *Quar. Rev. Biol.*, 3, 1928, facing 488; *A History of Psychology in Autobiography*, II, 1932, xvi.

MOSSO, ANGELO (1846-1910). *Clark Univ. Decentennial Celebration*, 1899, facing 383 (with signature).

MÜNSTERBERG, HUGO (1863-1916). (1) M. Münsterberg, *Hugo Münsterberg*, 1922, frontispiece (with signature). (2) *Ibid.*, facing 26 (with his students at Freiburg). (3) *Scient. Mo.*, 30, 1930, 120 (with signature).

NAFE, JOHN PAUL (1887——). *Psychologies of 1930*, xvii.

PAGET, RICHARD (1869——). *Psyche*, 8, 1927, frontispiece no. 29.

PARKER, GEORGE HOWARD (1864——). *Quart. Rev. Biol.*, 3, 1928, facing 502.

PAVLOV, IVAN PETROVITCH (1849——). (1) *Scient. Mo.*, 17, 1923, 394. (2) *Ibid.*, 620 (with son). (3) *Dedicated to Pavlov: 75th Jubilee*, 1924, frontispiece (with signature). (4) *Scient. Mo.*, 20, 1925, 330. (5) *Psyche*, 7, 1926, frontispiece no. 25. (6) I. P. Pavlov, *Lectures on Conditioned Reflexes*, Eng. trans., 1928, frontispiece (sketch, with signature). (7) *Ibid.*, facing 16 (three

photographs, outdoors and at play). (8) *Ibid.*, facing 320 (at operating table). (9) *Ibid.*, facing 368 (in study). (10) *Scient. Mo.*, 26, 1928, 446. (11) *Quar. Rev. Biol.*, 3, 1928, facing 505; *Scient. Mo.*, 29, 1929, 283; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 78. (12) *Scient. Mo.*, 29, 1929, 378 (with H. Cushing). (13) F. Fearing, *Reflex Action*, 1930, facing 282. (14) *Psychologies of 1930*, xviii.

PIÈRON, HENRI (1881——). (1) *Psyche*, 6, 1925, frontispiece no. 21. (2) *Wittenberg Symposium*, 1928, 8. (3) *Quar. Rev. Biol.*, 3, 1928, facing 505.

PILLSBURY, WALTER BOWERS (1872——). (1) *Wittenberg Symposium*, 1928, 8; *A History of Psychology in Autobiography*, II, 1932, xvi.

PINEL, PHILLIPPE (1745-1836). *Ment. Hygiene*, 14, 1930, suppl. following 462.

PRINCE, MORTON (1854-1929). (1) *Psychologies of 1925*, facing 199; *Ped. Sem.*, 32, 1925, frontispiece. (2) *Wittenberg Symposium*, 1928, 9. (3) *J. Abn. & Soc. Psychol.*, 24, 1929, facing 249.

REYMERT, MARTIN LUTHER (1883——). *Wittenberg Symposium*, 1928, 12.

RIBOT, THEODULE ARMAND (1846-1916). (1) *J. d. Psychol.*, 12, 1915, facing 260 (with signature). (2) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 200.

RIGNANO, EUGENIO (1870——). *Psyche*, 6, 1926, facing 40, no. 24.

ROBERTSON, GEORGE CROOM (1842-1892). J. Sully, *My Life and Friends*, 1918, facing 182.

ROMANES, GEORGE JOHN (1848-1894). *Quar. Rev. Biol.*, 3, 1928, facing 488.

ROYCE, JOSIAH (1855-1916). (1) H. James, *The Letters of Wm. James*, 1920, II, facing 134 (with Wm. James). (2) M. Münsterberg, Hugo Münsterberg, 1922 (with James and Palmer). (3) *Scient. Mo.*, 30, 1930, 118 (with signature).

SANDER, FRIEDRICH (1889——). *Psychologies of 1930*, xviii.

SANFORD, EDMUND C(LARK) (1859-1924). *Amer. J. Psychol.*, 36, 1925, facing 157 (with signature).

SCHNIERMANN, ALEXANDER LINOWITSCH (1899——). *Psychologies of 1930*, xviii.

SEASHORE, CARL EMIL (1866——). (1) *Psychol. Monog.*, 39, 1928, frontispiece no. 178 (a telephotograph). (2) *Wittenberg Symposium*, 1928, 9; *A History of Psychology in Autobiography*, I, 1930, xvi.

SEGUIN, EDOUARD (1812-1880). S. P. Davies, *Social Control of the Mentally Deficient*, 1930, facing 38.

SIDGWICK, HENRY (1838-1900). J. Sully, *My Life and Friends*, 1918, facing 278.

SMALL, WILLARD STANTON (1870——). *Quar. Rev. Biol.*, 3, 1928, facing 502.

SOMMER, KARL ROBERT (1864——). *Ment. Hygiene*, 14, 1930, suppl. following 462.

SOUTHARD, ELMER ERNEST (1876-1920). (1) *Ment. Hygiene*, 4, 1920, frontispiece. (2) *Ibid.*, 14, 1930, suppl. following 462.

SPEARMAN, CHARLES EDWARD (1863——). (1) *Wittenberg Symposium*, 1928, 9. (2) *Psychologies of 1930*, xviii; *A History of Psychology in Autobiography*, I, 1930, xvi.

SPENCER, HERBERT (1820-1903). J. Sully, *My Life and Friends*, 1918, facing 288.

STERN, LOUIS WILLIAM (1871——). (1) *Wittenberg Symposium*, 1928, 9; *A History of Psychology in Autobiography*, I, 1930, xvi.

STRATTON, GEORGE MALCOLM (1865——). *Wittenberg Symposium*, 1928, 10.

STUMPF, CARL (1848——). *A History of Psychology in Autobiography*, I, 1930, xvi.

SULLY, JAMES (1842-1923). J. Sully, *My Life and Friends*, 1918, frontispiece.

TERMAN, LEWIS MADISON (1877——). (1) H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 16. (2) *A History of Psychology in Autobiography*, II, 1932, xvi.

THORNDIKE, EDWARD LEE (1874——). (1) *Teachers College Record*, 27, 1926, facing 459 (with signature). (2) *Quar. Rev. Biol.*, 3, 1928, facing 502; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 102.

TITCHENER, EDWARD BRADFORD (1867-1927). (1) *Studies in Psychology; Titchener Commemorative Volume*, 1917, frontispiece (with signature); *Wittenberg Symposium*, 1928, 12. (2) *Amer. J. Psychol.*, 38, 1927, facing 489 (with sig-

nature). (3) *J. General Psychol.*, 1, 1928, frontispiece; *Scient. Mo.*, 26, 1928, 282. (4) *Ibid.*, 30, 1930, 121.

TROLAND, LEONARD THOMPSON (1889-1932). *Psychologies of 1930*, xix.

VERWORN, MAX (1863-1921). *Quar. Rev. Biol.*, 3, 1928, facing 488.

VON FREY, MAX (1852-1932). *Scient. Mo.*, 34, 1932, 478.

WARD, JAMES (1843-1925). (1) *Amer. J. Psychol.*, 36, 1925, facing 449 (with signature). (2) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 300.

WARREN, HOWARD CROSBY (1867——). (1) *Scient. Mo.*, 28, 1929, 39. (2) *A History of Psychology in Autobiography*, I, 1930, xvii.

WASHBURN, MARGARET FLOY (1871——). (1) *Amer. J. Psychol.*, 39, 1927, frontispiece (with signature). (2) *Wittenberg Symposium*, 1928, 10. (3) *Psychologies of 1930*, xix; *A History of Psychology in Autobiography*, II, 1932, xvii.

WATSON, JOHN B(ROADUS) (1878——). (1) *Psychologies of 1925*, facing 1; *Ped. Sem.*, 32, 1925, following 202. (2) *Psyche*, 7, 1927, frontispiece, no. 27 (with signature); *Quar. Rev. Biol.*, 3, 1928, facing 505. (3) W. B. Pillsbury, *History of Psychology*, 1929, facing 253; H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 160.

WEISS, ALBERT PAUL (1879-1931). (1) *Wittenberg Symposium*, 1928, 10; *Psychologies of 1930*, xix.

WITMER, LIGHTNER (1867——). *Clinical Psychology: Studies in Honor of Lightner Witmer*, 1931, frontispiece.

WOODWORTH, ROBERT SESSIONS (1869——). (1) *Psychologies of 1925*, 111; *Wittenberg Symposium*, 1928, 11; *Ped. Sem.*, 33, 1926, frontispiece. (2) H. E. Garrett, *Great Experiments in Psychology*, 1930, facing 134; *Psychologies of 1930*, xix; *A History of Psychology in Autobiography*, II, 1932, xvii.

WUNDT, WILHELM MAX (1832-1920). (1) G. S. Hall, *Founders of Modern Psychology*, 1921, facing 311. (2) *Scient. Mo.*, 11, 1920, 383; 30, 1930, 122 (with signature). (3) *Arch. f. ges. Psychol.*, 40, 1920, i (death-bed sketch). (4) *Ibid.*, ii (bronze plaque); E. G. Boring, *A History of Experimental Psychology*, 1929, frontispiece (bronze plaque). (5) R. H. Gault and D. T. Howard, *Outline of General Psychology*, 1925, facing 400. (6) *Psychol. Rev.*, 28, 1931, facing 153 (with signature). (7) *Amer. J. Psychol.*, 44, 1932, facing 615 (with signature). Cf. *ibid.*, 32, 1921, 177.

YERKES, ROBERT MEARNS (1876——). (1) *Quar. Rev. Biol.*, 3, 1928, facing 505. (2) *A History of Psychology in Autobiography*, II, 1932, xvii.

ZELLER, EDUARD (1844-1908). G. S. Hall, *Founders of Modern Psychology*, 1912, frontispiece.

ZIEHEN, THEODOR (1862——). *A History of Psychology in Autobiography*, I, 1930, xvii.

ZWAARDEMAKER, HENRICK (1857-1930). *A History of Psychology in Autobiography*, I, 1930, xvii.

We limited our search for portraits to the Columbia psychological library because we had to set ourselves a finite task, and also because the library, which is large, probably includes all the books and periodicals that are available to most psychologists. If any omissions are brought to our attention, we shall be glad to rectify them in the supplementary lists which we hope to publish from time to time.

Columbia University

L. V. BERGMAN
KARL M. DALLENBACH

DR. WILCOX ON "THE RÔLE OF FORM IN PERCEPTION"

In the July number of this JOURNAL,[1] Dr. Wilcox criticized the conclusions reached by Fehrer and myself regarding the rôle of form at the lower threshold

[1] W. W. Wilcox, Helson and Fehrer on "The rôle of form in perception," *op. cit.*, 44, 1932, 578-580.

of vision.[2] We found that it required 15 times more illumination to perceive form, even though the form seen did not coincide with the stimulus-form, than to report on the mere presence of light, and 25 times more illumination to name the stimulus-form correctly than was required to perceive the vague haze characteristic of liminal vision. We were unable to demonstrate the superiority of any one form over any other according to numerous criteria of goodness since no one form ranked first in a majority of the tests. We did not find that the circle possessed a lower threshold than other forms or that it was superior according to other criteria. Dr. Wilcox found that his *Os* "invariably" reported their liminal experiences shaped as circles. He believes that forms are first and primary in experience and that the perception of light at the threshold is always of a circular form. He implies that circles should have the lowest thresholds, should be reported first when any form at all is perceived, and hence should be the best of figures in perception. Dr. Wilcox's criticisms of our work depend (1) upon questions of fact, and (2) upon misinterpretations of our position as well as a neglect of the evidence we offered in support of our conclusions.

Are forms, circles, perceived at the lower threshold of vision? Our findings were not unique: they corroborate the work of Poppelreuter, Gelb and Goldstein, with abnormal subjects, and Seifert in vision, that of Zigler and his students both in vision and on the skin, and that of Kleitman and Blier in vision.[3] A writer of nearly 30 years back may be regarded as impartial. Piper pointed out in her well-known work on dark adaptation that form perception is lacking at the lower threshold. Instead of form, she says, "Man hat vielmehr den Eindruck eines unbestimmt umgrentzen schwachen Lichtnebels, der manchmal nicht ohne weiteres von den ganz ähnlich sich bietenden subjektiven Lichterscheinungen unterschieden werden kann."[4] If Dr. Wilcox choses to call such experiences formed it is evident he is using the term form as no other writers, even some configurationists included, use it. For him anything and everything is formed and the word loses all significance both in this and any other discussion of form. We were criticizing a view of the efficacy of form which employed the more ordinary meaning of the word. Dr. Wilcox may use words as he likes but he should realize it was not his meaningless term that we were concerned with.

Dr. Wilcox accuses us of the stimulus-error in attempting to account for our results.[5] Since no particular form was consistently preferred in our experiments while in his the circle was invariably perceived it does not require much thought to decide whose *Os* were biased. We avoided the stimulus-error by requiring our *Os* to report what they first saw without regard to the stimulus being exposed. Dr. Wilcox not only neglects the measurements given for the threshold values of the various figures we used but also seems to be unacquainted with our actual mode

[2] H. Helson and E. V. Fehrer, The rôle of form in perception, this JOURNAL, 44, 1932, 79-102.

[3] A brief résumé of the work of these people, with the exception of Seifert, was given in our article, *op. cit.*, 83 f. Seifert's article will be found in *Zsch. f. Psychol.*, 78, 1917, 55-144.

[4] H. Piper, *Über Dunkeladaptation, ibid.*, 31, 1903, 187. Piper's reference to form is unequivocal: "Es ist nicht mehr möglich unter solchen Bedingungen die *Form* des Erregers zu erkennen."

[5] Wilcox, *op. cit.*, 379.

of procedure, all of which was fully explained in the article which he criticizes.

The only evidence our critic has offered to support his contentions are the reports of his *O*s gained, so far as his published statements go, by the simple expedient of asking them to report what they saw near the threshold. There can be no scientific discussion of an issue merely hinging upon the reports of one set of *O*s as against those of another. Our conclusions were based upon measurements and several methods of calculation designed to bring out the various properties a form might have. In none of them did the circle take first place or even second. Can Dr. Wilcox dismiss such facts as these with the mere assertion of stimulus-error? Why did the circle rank fourth among the figures reported as first seen if it is invariably present at the threshold? Why was it necessary to make a larger number of exposures for the circle than for rectangle, square, and triangle in order to get 50 correct reports of form if circles tend to be seen more readily than other figures? Why was the rectangle called correctly at a lower illumination than the circle if circles are invariably seen at the threshold? Why did not the circle give a lower threshold than the other figures if it is present coincidently with the first appearance of light? Why were three other figures reported as present phenomenally (without regard to figure actually exposed) more times than the circle if the circle typifies liminal experience? We are entitled to a consideration of such facts as these in any criticism of our work but Dr. Wilcox seems unaware of them.

One misstatement, a *tour de force*, requires correction. Dr. Wilcox states that we criticize the view "that perceptual and bodily responses of the organism *form* a whole, a unit, a configuration."[6] This is absolutely untrue. This criticism is wholly of his own making. We are in hearty accord with this proposition which must be sharply distinguished from the proposition, often made by the narrower group of configurationists, that form as such is first and primary in experience.

We do not believe that Dr. Wilcox's experiments and criticisms, based as they are upon a conception of form so broad as to be devoid of meaning, either impugn our conclusions or lend support to a generalization far too wide and sweeping in the light of all the evidence. Our work concerned the testing of an hypothesis under special conditions where it was found wanting. It is Dr. Wilcox's privilege to change the hypothesis but the facts stand.

Bryn Mawr College HARRY HELSON

THE EFFECTS OF DISTRACTION

After completing a series of experiments in 1918, Cassel and Dallenbach concluded that "the effect of 'distraction' upon the sensory reaction is equivocal. The distractor may inhibit, and lengthen the reaction; it may facilitate, and shorten the reaction; or it may become habitual, and have no effect at all."[1] Reports by their observers indicated that these discrepant results might be dependent upon the temporal relations of the distractor and upon the attitude of the observer during the distraction.

Writers of textbooks seem to have ignored these conclusions, in spite of the implications respecting the effects of distraction in general; and their challenge for

[6] *Ibid.*, 578.

[1] E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this JOURNAL, 1918, 29, 129-143.

further experimental work seems to have been neglected. To attempt an experimental verification, the writer trained 5 *O*s until they were able to perform at a high and constant rate the simple motor skill which serves as the basis for this part of the projected experiment. The *O*s were confronted with a board containing 100 metal pegs arranged to form a square; and they were instructed to remove these pegs one at a time and to place them as rapidly as possible in a like arrangement on a duplicate board. After a long training period, the *O*s engaged in experimental determinations in which 'distraction' and control runs were interspersed. In the former they were subjected to a continuous tone of 260 cycles, while the control runs were carried on without this distractor. (Since *E* was outside the room and invisible to the *O*s it was assumed that he did not constitute an important distractor.)

Data now available for a large number of trials fail to exhibit any statistically reliable difference between times for observers engaged in control series and the same observers in 'distracted' runs. This would seem to support the contention of Cassel and Dallenbach that continuous distractors are readily habituated. Although variations in attitude are reported, there is no evidence of co-variability of attitude and time required for a particular run. Apparently the conclusions of Cassel and Dallenbach may not be extended to cover 'motor' reactions. A later publicaiton will report our results with a task which involves judgment and which will, we hope, more closely approximate the conditions of a 'sensory' reaction.

Cornell University JOHN G. JENKINS

THE FORTIETH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Fortieth Annual Meeting of the American Psychological Association was held at Cornell University, Ithaca, New York, on Thursday, Friday, and Saturday, September 8, 9, and 10, 1932. Registration figures show a total of 612 persons in attendance classified as follows: 121 members, 250 associate members, 31 newly-elected associates, and 210 persons not affiliated with the Association.

At the business meeting, 19 Associates were transferred to the status of Member and 193 Associate Members were elected. The Secretary announced that the Forty-first Annual Meeting will be held at the University of Chicago on September 7, 8, 9, 10, 11, 12, and 13, 1933. Professor Harvey A. Carr was appointed as a member of the Executive Committee for 1932-1933, and John E. Anderson, Franklin Fearing, and the Secretary were elected as the Program Committee for 1933. It was voted to amend Section 2, Article VIII, of the by-laws by adding the following sentence: "In case a husband and wife both hold membership, one may elect to receive the Psychological Bulletin instead of the abstract journal of the Association providing an additional payment of two dollars a year be made to the Treasurer when the annual subscription is paid." Newly-elected officers are: President, L. L. Thurstone; Directors, Edward A. Bott and Henry E. Garrett; Division of Anthropology and Psychology, N.R.C., Walter S. Hunter and Herbert Woodrow; S.S.R.C., L. L. Thurstone; A.A.A.S., Harvey A. Carr and Walter F. Dearborn. It was voted that the American Psychological Association, Inc., should apply for membership in the Inter-Society Color Council. Clarence E. Ferree, A. T. Poffenberger, and Forrest Lee Dimmick were appointed as delegates to the Inter-Society Color Council. The

Association voted to establish a new committee to be known as the Committee on Research in the Social Sciences consisting of the three representatives on the S.S.R.C., the President and the Secretary of the Association as *ex officio* members and two members to be appointed by the Council of Directors. R. S. Woodworth and John E. Anderson were appointed to complete the membership of this newly-established committee. Following acceptance, with thanks, of the report of the Committee on Psychology of the National Advisory Council on Radio in Education, it was voted that the Association express its appreciation for what the Committee has accomplished and especially to thank Dr. Walter V. Bingham for the effective way in which he planned and executed the nation-wide broadcasting of psychology lectures during 1931-1932.

In commemoration of the fortieth anniversary of the American Psychological Association, Joseph Jastrow, one of the founders of the Association and its first Secretary-Treasurer, spoke briefly concerning the events leading to the launching of the Association in 1892. A cablegram to the Directors of the Laboratory of Psychology of the University of Leipzig commemorating the one hundredth anniversary of the birth of Wilhelm Wundt and congratulating members of the Leipzig Laboratory for carrying on so worthily the work of their great master was authorized and transmitted while the meetings were in session.

Seventeen sessions for formal papers were scheduled and a total of 106 papers were presented. If one assumes that the program of the Association represents a cross-section of current psychological research, then it would appear that research with animals and young children as subjects is in the ascendency. Papers on mental tests as such were conspicuous by their relative absence. Nevertheless, it would be premature to conclude that research interest in mental testing is declining. Rather, this type of research acvtivity seems to be directed toward the measurement of personality and character traits as indicated by the nature of the papers included in the session on Personality and Character. In contrast with previous meetings, there were several papers on applied or technological psychology. The dominant characteristic of the program as a whole may be summarized by saying that the papers reflected a widespread interest in objective, behavioristic experimentation with emphasis upon physiological analyses of behavior.

Two of the seventeen sessions were devoted to clinical psychology. At the annual meeting of the Section on Clinical Psychology, Edgar A. Doll was elected Chairman and Richard H. Paynter was elected a member of the Executive Committee for 1932-1935. A total of 230 persons are now listed as being affiliated with the Section on Clinical Psychology.

On Friday afternoon at 4:30 P.M., a Round Table on *Radio in Education* was held with Paul S. Achilles as Chairman. Informal reports of research studies concerning the effectiveness of radio addresses were made. Announcement was made that details of researches in this field are being assembled by Dr. F. H. Lumley, in charge of radio research, Ohio State University.

On Thursday and Friday afternoons, the Department of Psychology of Cornell University was host at two delightful teas held in the Laboratory of Psychology, Morrill Hall.

On Friday evening, the Presidential Address, "Age and Human Ability" was delivered by Dr. Walter R. Miles. Pointing out that the science of psychology has

shown special vigor in those regions where the age aspects of behavior are emphasized, Dr. Miles argued that life-age might well become the entity for study in psychology just as space-time is the subject for study in physics. It was also shown that the psychological study of the effect of age on human abilities has been largely confined to the first two or two and a half decades of life, thus leaving relatively untouched five or six decades of maturity and later maturity. Without minimizing the importance of previous studies on adults, Dr. Miles devoted his address chiefly to a general presentation and discussion of the extensive series of studies on later maturity which he has been directing at Stanford University. An outline picture of age changes in the mental abilities so far investigated was presented. In general it appears that sensori-motor functions show early and decided decline, although in some instances the decline, can be compensated for by other and more complex behavior. Abilities which can be designated as more nearly psychological or mental tend to reach their peak at later life ages. The results so far obtained suggest the possibility of constructing a psychological map of human life-age.

In commemoration of the occasion as being the Fortieth Annual Meeting of our Association, Dr. Miles prefaced his address by an appropriate expression of gratitude to the American pioneers in psychology who organized our Association in 1892, many of whom are still living.

To provide well-nigh perfect facilities for a scientific meeting at which 612 persons registered is an achievement which can be duplicated by few institutions in this country. This was possible at Cornell not only because of the physical lay-out but also because of meticulous planning and efficient executive action. Professor Bentley and his associates have earned the heartfelt gratitude of our membership.

University of Minnesota DONALD G. PATERSON

THE WUNDT CENTENARY

The Institute of Psychology of the University of Leipzig celebrated the centenary of the birth of Wilhelm Wundt on October 29 and 30, 1932. The actual centenary was the 16th of August. The celebration consisted of three main functions, a demonstration of the work being done in the Institute, a formal commemoration meeting, and a banquet with less formal tributes.

The laboratory demonstration was introduced by the Director, Professor Krueger who outlined the general policy of the laboratory. Professors Klemm, Vokelt, and the assistants showed the apparatus being used for the work carried on under their direction.

The commemoration meeting was held in the aula. A number of shorter tributes were made by university and city dignitaries and by representatives of psychological societies, then Professor Krueger presented an extended account of Wundt's contributions to psychology, and an estimate of his work and position in the world of science.

At a closing banquet many brief testimonials were given and greetings read from individuals and psychological organizations in all parts of the world. The resolutions of the American Psychological Association were especially emphasized.

On October 30, Fräulein Wundt invited those present to her home to see her father's collected works and manuscripts, note books, and various mementoes. This was one of the most significant events of the celebration.

W. B. PILLSBURY

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Purposive Behavior in Animals and Man. By EDWARD CHACE TOLMAN. New York, Century Co., 1932. Pp. xiv, 463.

"This book has grown out of an experimental interest in animal learning grafted upon an arm-chair urge towards speculation." The book is appropriately dedicated to the white rat (without whose services it could not have been written) but it might equally well have been dedicated to the speculative philosopher.

Purposive behaviorism is a complete system of psychology most closely related to configurationism and perhaps a sub-variety of the Gestalt doctrine. Purpose, cognition, expectation, and similar terms are used to describe behavior without any mentalistic implications. These terms characterize behavior as *molar* but not necessarily behavior as *molecular*. This distinction between the molecular and molar views of behavior is clearly drawn. The molecular view, referred to Watson, defines behavior in terms of underlying physical and physiological details. The molar view, presented by Professor Tolman, recognizes that behavior has properties of its own which are not deducible from physiological processes. Among these 'emergent' properties are purpose and cognition—"behavior as behavior reeks of purpose and of cognition (p. 12)."

The ultimate goals of behavior are physiological states of quiescence which are sought or disturbances which are avoided. The animal's behavior expresses subordinate demands and readinesses for specific goal-objects. Some goal-objects are demanded more than others. A shift of incentive demonstrates in the learning curve the great importance of the goal-object in the learning process. A great deal of important experimental work is summarized.

Purposive behaviorism concerns itself only with 'docile' behavior, *i.e.* with behavior which is modifiable as distinct from bare reflexes and tropisms. The leading theories of learning—the conditioned reflex theory, trial and error, the Gestalt theory—are critically examined and Professor Tolman's own views clearly presented. A distinction is drawn between stimulus laws and capacity laws.

Behavior does not go on *in vacuo* but rests upon basic environmental conditions called 'behavior supports,' such as the black and white objects to be discriminated or the alley of the maze. The rat behaves as if the maze were of a particular sort and a slight change of experimental conditions may disrupt behavior. In this objective sense the rat is said to show expectation. Sensory supports are called 'discriminanda' and motor supports 'manipulanda.' Other concepts such as perception, mnemonization and inference are developed.

In one part of the book entitled "Rats, Cats, Apes, and Men" an attempt is made to bridge the gap between 'the rat in the maze' and 'the psychologist in the laboratory.' Here the problems of insight, conscious awareness, and inventive ideation are tackled. Conscious behavior for Tolman is not fundamentally different from other behaviors. Consciousness appears to be a sampling reaction, a 'running-back-

and-forth.' The author is somewhat reluctant over the 'shameful necessity' for raising the question of awareness, for the behavior of rats was described without talking about consciousness. Howbeit men insist that they are conscious and have ideas. Some hypothesis is needed and the 'running-back-and-forth' hypothesis is developed.

The discussion of the psychologist in the laboratory is perhaps the weakest part of the book. Introduced apologetically the topic appears to have little genuine interest for the author. Introspection is speech behavior. The distinction between *Kundgabe* and *Beschreibung* is discussed. The latter—the 'pure' or 'true' introspection—is said to be an abortion because there is no stuff to a 'running-back-and-forth.' 'Raw feels' do not and cannot get across. If there be 'raw feels' correlated with 'discriminanda-expectations' no science can touch them. Images, like sensations, are 'discriminanda-expectations' but with internal stimuli. Feeling and emotion are referred to briefly.

The part of the book entitled "Motivation and Learning" contains much valuable material. The ultimate motivations of behavior are appetites and aversions. The former are demands for physiological quiescence, the latter demands against physiological disturbances. Second order drives and personality mechanisms are discussed. The term 'instinct' is retained for types of response or determinants of behavior which are common to practically all members of the species.

In a final section of the book the system is considered as a system and a special summary is presented for psychologists and philosophers. Tolman's system of psychology is diagramed and critically related to other systems.

From the formal point of view the presentation suffers from the introduction of too many neologisms. The difficulty is illustrated by the following quotations which are (perhaps unfairly) taken out of their context: "These discriminanda-expectations, these 'sensations,' become conscious and constitute the 'conscious feeling' of pleasantness or unpleasantness, if the organism starts to behavior-adjust to running-back-and-forth with respect to them (p. 261)." "By virtue of his initially active means-end-readinesses and discriminanda- and manipulanda-readinesses he can behavior-adjust, initially, to certain portions of the field and then, if he is fortunate, the requisite new discriminanda-, manipulanda-, or means-end-readinesses (*i.e.* sign-gestalt-readinesses and -expectations) will 'click' (p. 371f.)." Many of the ideas could be clothed more simply. A glossary is provided as an aid.

Despite the neologisms the general style is delightfully informal and on the level. The book is distinctly a contribution to psychology and is well worth the careful study of serious students. It is an authoritative statement of Professor Tolman's views and system of psychology.

University of Illinois PAUL THOMAS YOUNG

The Psychological Register: Vol. III. Edited by CARL MURCHISON with the coöperation of an international board and the assistance of Luberta Harden. Worcester, Mass., Clark University Press, 1932. Pp. xi, 1269.

The *Register* furnishes the bibliographies of psychologists the world over, together with certain biographical data. This volume, marked Vol. III, is a revision of the 1929 volume, which is now to be known as Vol. II. The designation Vol. I is reserved, we are told in the Preface, for a volume, still in course of preparation, that will be devoted to the psychologists who died before the initiation of the series.

The present edition is a vast improvement upon the earlier. Not only is it more complete (40 countries are represented as compared with 29, and approximately 2400 names are entered as compared with 1250), but it is more competently and carefully prepared and edited. Most of the defects of the 1929 edition (cf. the review in this JOURNAL, 42, 1930, 153-156) have been corrected. The greater geographical range has in the main been achieved by a more logical system of classification. Canadian psychologists, who were listed with those of the United States under the heading 'America,' now enjoy a separate section, as do also the psychologists of the other dominions and possessions of the British Empire. Only the psychologists of England, Scotland, and Wales are now listed under the heading 'Great Britain.' The increase in number of names entered is due in part to the greater time and care taken in preparation, and in part to the enlargement and the wider geographical distribution of the coöperating board (43 members from 36 countries as against 8 members from 8 countries).

The book is arranged alphabetically by country and, within these divisions, by name. It closes with an index of names, which enables the user to find an entry when he does not know the country under which it appears. The arrangement is the same as it was in the earlier edition, and it probably represents the permanent mold into which the Series is to be cast. If so, this is very unfortunate; for an alphabetical arrangement accompanied by a geographical index—as in the standard *Who's Who*—would be much more serviceable. The reader would never have to turn to the index to find an entry, and the geographical index would permit a more detailed classification—by city or town as well as by country. The adoption of this form would also emphasize the obvious need for translating into English the biographical information now printed in a polyglot of English, Spanish, German, French, and Italian, and occasionally in other less known languages.

The bibliographies also are cited in a variety of languages. English, French, German, Italian, and Spanish titles are given in the original language. No rule, however, is followed in respect to other languages. Sometimes the titles appear in the original; at other times they are translated into English, German, or French; and all this happens within the same division, to the same author, and on the same page! (Cf., pp. 1012 ff., 1234 ff.) Doubtless, copy comes to the editors in this medley of tongues; but certainly, if all the titles are not to be put into English—as is preferable—some consistent procedure could and should be adopted.

In marked contrast to the earlier edition, the references now follow a uniform style of citation. With but few exceptions, date, volume, and inclusive pages are given. The limiting dates, however, are not uniform: while most of the bibliographies stop with 1931, some of the American entries extend into 1932. Similar variations appeared in the earlier edition. Yet good editorial practice demands that the limiting date be the same for all.

The present volume has the same format as the earlier edition. It is printed in double columns of 8 pt. solid type. The authors' names are printed in boldface—surnames in capitals, given names in capitals and lower case. The printers of the volume, lacking some of the matrices necessary for this work, attempted to meet the requirements by hand-setting. This is shown by the fact that the accents and diacritical marks used in printing the surnames are set on 6 pt. letters (see, for example, pp. 666, 992, 1042); and also by the fact that the umlauts are frequently

off center (compare, for example, pp. 832 and 834, 850 and 851) and of different sizes (compare, for example, pp. 586 and 587, 788 and 789). These defects, which could have been remedied easily and at slight cost, give the page an unsightly appearance and also violate the canons of good printing. The editors of the volume should not have allowed their printers to use this subterfuge.

A number of names are incorrectly placed; for example, John Adams, p. 896; J. M. Baldwin, p. 17; John Greig, p. 931; F. C. S. Schiller, p. 967; E. W. Scripture, p. 436; L. Urwick, p. 875. As the biographical data clearly indicate, these men reside in countries other than those of their listing. Most of the misplacements may be accounted for on the ground that the individuals are placed in the countries of their origin instead of residence; but if that principle be accepted, then many other changes would have to be made if consistency is to be achieved. Questions concerning place of listing would have been avoided had the single alphabetical arrangement mentioned above been used. Here then is still another reason for its adoption.

K. M. D.

How To Interview. By WALTER VAN DYKE BINGHAM and BRUCE VICTOR MOORE. New York, Harper & Bros., 1931. Pp. xiv, 320.

The interview, or "conversation with a purpose" (p. 3), is shown to have so wide use and so much variability in methods and applications as to warrant careful study of the uses, techniques, and dependability of results. "Regarding pitfalls of interviewing and the best ways of avoiding them, there exists a wealth of hard-won practice and tradition not only among industrial investigators and employment managers, but also among physicians and psychiatrists, lawyers skilled in questioning, police inquisitors, salesmen, journalists, teachers, vocational counselors, and particularly social workers. Their practices and techniques it is our purpose to scrutinize, and to compare them with the results of experimental studies" (p. ix). This is the authors' main problem. They sought to improve the techniques and to avoid the pitfalls, to the end of obtaining more reliable data by means of the interview. The study was made possible by a grant from the Social Science Research Council.

In the investigation, conferences were held with sixty experienced interviewers and in some instances "their actual interviews were observed." Hypotheses for experimental tests grew out of differences in views and practices. The authors regard the testing out of methods and criteria and the improvement of techniques as more important, at the present stage of development, than the specific facts and data obtained. The interview, it should be noted, is used not only to secure information about attitudes, beliefs, interests, etc., but also to motivate and to instruct people. The interviewer, to save time, to prevent omissions, to avoid confusions and to elicit coöperation, must prepare his questions carefully before hand, must be thoroughly conversant with his problem, must follow a definite plan and outline, attend carefully to the precise wording of key questions and other such details: but above all he must be objective minded, and manifest a "straightforward honesty and frankness" that will engender respect and confidence on the part of the interviewee.

Two sorts of error are found, the variable, easily detected by the usual statistical methods, and the constant error. In the latter the statements not only vary one from another, but in general tend to vary in one direction from the facts they purport to describe; they are easily detected in "the scatter of observations." The causes of

constant errors may be located in the suggestibility or "leading" tendency of certain questions, in bias of persons interviewed, in certain mental sets of the investigator, etc., and may, on investigation, lead to better technique. Such studies by the investigators themselves are valuable. Chapter 2 is devoted to the systematization of the vocational interview, giving sets of aids in the way of blank forms to be filled out by the applicant, others to be worked out jointly by applicant and counselor, and still others to be filled out by the counselor only. The first of these call for present occupational plans, home and family circumstances, physical and mental condition, educational and work histories, vocational interests, etc.; the second covers the same topics from the standpoint of appraisal of the assets and liabilities of the applicant, while the last calls for recommendations, impressions and opinions, and scores made on certain tests. A manual of directions for counselors is also briefly outlined and described. The next chapter is devoted to the practical matters of learning how to interview and the giving and discussing of 52 suggestions. In a chapter on The Employment Interview one gets a view of the immensity of interviewing not only to help persons find the right job, but also to increase their earning capacity and to increase their comforts and happiness in the work. There are interviews for job analysis, for follow-up and adjustment of persons placed, for instruction, and many other purposes, and, finally, interviews when the employee leaves or is dismissed. The latter often furnish useful information regarding working conditions, attitudes of workers, etc. Pressure of work reveals the fact that these functions of interviews must often be imperfectly performed. In 1929, for instance, R. H. Macy & Co., New York, interviewed for employment 176,000 applicants of whom only about 13,000 were employed (1 in 13)! In such interviewing there is first a short sifting interview of ½ to 3 minutes followed by longer interviews, the filling out of application blanks, and probably even some tests, of the more promising candidates. Considerably more care is taken when one is to be promoted or appointed to an executive position. Sales managers fail to agree closely on individuals in brief personal interviews, the correlation of anyone's ratings with the ratings by others usually running from 0.55 to 0.85 (which is about 0.16 to 0.47 better than a pure guess). But even this is a very considerable improvement over results of earlier tests of similar kinds.

Other chapters deal with interviews in industrial relations, in marketing studies and commercial surveys, in social case-work, in education, in mental clinics, in journalism, and in law, and with evaluations of results. The last three chapters (14–16) present in considerable detail two researches in the actual use of interviews for industrial fact-finding. In the first research employees were interviewed to determine their attitudes toward the provisions of an employment guarantee plan, and in the second one textile workers were interviewed during a strike with a view to ascertaining their attitudes toward arbitration. In these latter (experimental) studies, results from cumulative groups of increasing size were correlated with those from all the interviewees, to ascertain approximately the number of interviews necessary for fairly reliable results. The authors find that the interview, skillfully used, has its greatest value in ascertaining the attitudes and feelings of the interviewees. Facts concerning external physical events and dates of past occurrences were less satisfactorily obtained. Divergences among results secured by different interviewers were accounted for to a considerable extent by the different compositions

of the groups interviewed by the several interviewers—nationality, sex, membership in different workers' organizations, etc.—and it was found that great care should be taken that the questions be made clear as to their implications or meaning, and that a final checking with the interviewee of his replies contributed considerably toward this end. Results showed that 200 "well selected and carefully directed interviews are highly indicative of what can be obtained from two or three times as many." The accuracy of results were checked both by using records made accessible and by data from a secret ballot. One may well query whether, and to what extent, the more reliable results obtained as to attitudes and feelings of the interviewees as compared with those as to memories of events may be due to a greater limitation of possible replies and to the more stereotyped forms of reply and the more frequent discussion (social inter-stimulation) in the case of attitudes. The only test of the validity of reports relating to attitudes and feelings would seem to be consistency among the replies.

The clear and interesting manner in which these studies have been presented by the authors will undoubtedly tend to stimulate further research on the interview. A valuable feature of the book is a classified bibliography of 338 titles.

J.P.

Contemporary Schools of Psychology. By R. S. WOODWORTH. New York, Ronald Press, 1931. Pp. vi, 332.

Woodworth's own words characterize this book. It seeks to "provide an objective view of contemporary psychology so far as concerns the schools and their illuminating views of humanity" (p. iii.). It is argued that the view shall not be exhaustively critical but for the most part expository. The setting for the exposition is created by a brief historical chapter which says that the history of psychology is a continuous series of revolts against an "established order." In 1900, the date which separates the "contemporary schools" from the history of psychology, the science is said to have subscribed to a study of consciousness, to the method of analysis, to associationism and to a physiological bias. Since 1900 the chief schools, developed by the natural tendency of ambitious young men to "pick flaws," are behaviorism, psychoanalysis, purposivism, existentialism, and Gestalt psychology. The chapter closes with a prophecy of the sort of unity to be found in the science, a prophecy which does not find fulfillment, however, in the last chapter.

Chapter 2 makes a survey of the existential school of psychology. This school is described as the logical descendant of the introspective school and of the method of impression. The development of the school is traced through the work of Wundt, Ebbinghaus, Müller, the Külpe school, and Titchener. Chapter 3 tells the story of behaviorism which is, for the most part, the story of Watson. This school is seen to have developed out of (1) functionalism (James, Dewey, Angell), (2) the performance psychologies (Galton, Ebbinghaus, Cattell, McDougall), (3) animal psychology (Thorndike), and (4) conditioned and unconditioned reflexes (Bekhterev, Pavlov). The last part of this chapter considers the later development of Watson's point of view and the work of other behaviorists such as Max Meyer, Weiss, Hunter, and Lashley. Altogether the author writes about behaviorism with more competence and understanding than he displays with respect to existentialism.

Chapter 4 considers Gestalt psychology. This is another revolt against "the established order," viz., associationism; but whereas behaviorism reacted against intellec-

tualism and consciousness, configurationism reacted against analysis. A short summary of the origins of Gestalt psychology (Ehrenfels) is followed by an account of some of the experimental work which supports the configurational school, *i.e.* in the field of perception (including figure and ground), animal behavior (Koffka, Köhler), and learning.

Chapter 5, the longest in the book, tells of the development and of the program of psychoanalysis and related schools. The story begins with Charcot, the early struggles of Freud, and with the initial work of the psychoanalytical school. Several of the concepts central to the school are then described, viz., repressed infantile sexuality, motivation, polarity, the subconscious, and the like. In a brief appreciation of the movement, Woodworth ventures the opinion that Freud's psychology can scarcely "rank with the great scientific theories which coördinate existing knowledge and serve as guides to further discovery" (p. 161). In spite of its "freshness and daring," its concepts are said to be retrograde rather than forward-looking. The remainder of the chapter is devoted to a brief account of Adler and Jung.

Purposivism or hormic psychology is the topic of Chapter 6, and as is almost inevitable, it centers largely upon the work of McDougall, especially with reference to instinct. It seems to the reviewer that Woodworth rightly challenges McDougall's assertion that hormic psychology puts the science at a parting of the ways. The challenge is offered in terms of the proposition that "purposes" within behavior are of one character whereas a general philosophy of purpose lies outside of psychology and is to be accepted or rejected without any prejudice to the science itself.

The last chapter (Chapter 7) is an exceedingly brief account bearing the title, "The Middle of the Road." The middle of the road turns out, in part, to be a convenient place to mention Spearman, Myers, Spranger, Stern, Claperède, Benussi, de Sanctis, Kiesow, Piéron, Dumas, and others; and, in part, a place where it can be said that hundreds of psychologists owe no primary allegiance to any of the schools.

We thus see that Woodworth has stuck to his program of giving an objective but uncritical exposition of the schools of psychology. It is as though he had written an extended review of the *Psychologies of 1925* and of *1930* together with occasional references to the first volume of the *History of Psychology in Biography* so that beginners might have a general perspective of the science. The major conclusion is that younger psychologists should obey the injunction of a negro melody and "keep in the middle of the road," although no definition of this position is offered save that which is suggested by the fact that it is impossible to locate all the members, say, of the *American Psychological Association* under any one of the banners of the five schools named. The book is engagingly written and often reveals the genial spirit of the author; but it leaves one with a deep yearning for a major piece of systematic writing.

University of Illinois COLEMAN R. GRIFFITH

Race Psychology. By THOMAS RUSSELL GARTH. New York, Whittlesey House (McGraw-Hill), 1931. Pp. xiv, 260.

This volume of Garth's is the first to appear in which presumably the aim is to give a systematic survey of the problems, methods and results of experimental race psychology. Undoubtedly a book of this sort could be a valuable contribution to a

field where the canons of experimental science have too often been neglected in attempts to accumulate "results" to prove an *a priori* hypothesis. Perhaps the reviewer expected too much of the book, especially in view of the statement in the preface that the author "has endeavored to keep the general reader in mind as much as would be consistent with a scientific handling of the materials." At any rate, his general feeling on reading the book has been one of disappointment. The "general reader" has been kept too much in mind, particularly the reader saturated with the backwash of sociological theorizing which has captivated the imagination of the "tenderminded" during the past decade.

It is debatable whether or not a book on race psychology designed to serve as an elementary text-book or as a popular treatise for the non-psychological public should be written at the present time. The non-technical reader lacks the background requisite to the proper evaluation of the masses of data presented all too sketchily and uncritically in Garth's book. It is misleading to give the impression that an unambigous body of factual material exists which can be "interpreted" in support of an environmental explanation of race differences in performance on psychological tests. The proper scientific attitude in this field at present is one of agnosticism, an attitude which is alien to the "popular" reader. What is needed in race psychology is not an uncritical compendium of test "results" with interpretations of the sort referred to, but rather a discriminating, critical appraisal of the problems, methods, and results.

Space limitations prevent a detailed consideration of the subject matter of the book. In general, an attempt is made in the first two chapters to state the author's conception of the essential problem of race psychology, namely the question of the relative effects of heredity and environment in the causation of observed behavior differences. A very elementary survey of the methods and present status of studies of "mental heredity" is presented. These introductory sections are followed by chapters on the methods and results of experimental work on race differences in intelligence, color preferences, musical talent, personality, fatigue, and community of ideas. A final chapter on the evaluation of races epitomizes the author's position on the causes of race differences. Environment and "racial mobility" are held to be the essential causal factors.

Perhaps the two best features about the book are a bibliography of 196 titles and a summary table listing the following facts about the experimental studies of race differences: date, investigator, races compared, number of cases, results. A summary of results in brief tabular form would necessarily be fragmentary and, in view of the absence of a common unit in the several studies for expressing amounts of difference, often misleading and meaningless. A few examples might be cited: "94% of Indians below age," "whites excel Negroes," "Negros two years younger," "performance of Negroes ⅔ that of whites," "Negro deficiency 88 quotient units" (this is obviously an error), "Whites have stronger personality 15.4%." Such vague and varied comparative measures illustrate the desirability of the use of a standard unit for expressing race differences, a point made several years ago by Peterson, and which Garth appears not to have appreciated. In fact Garth urges "the use of the I.Q. in expressing the comparative intelligence of racial groups," despite the difficulties which he must have encountered in constructing his summary table. One feels almost apologetic in pointing out the obvious and numerous objections to the

use of gross I.Q. difference as an index of "comparative intelligence of racial groups." The I.Q. is not a "score" (*i.e.* a direct index of performance), it depends upon various questionable procedures as regards standardization when used to compare groups heterogeneous for race and age, and gross I.Q. differences cannot possibly indicate the amount of difference between two specific groups in the only sense in which "amount of difference" has any statistical meaning; namely, in terms of normal frequency curve area. Garth believes that the I.Q. is an "easily understood measure," as an index of comparative mental status of different groups. The reviewer would like to insist that gross I.Q. difference alone cannot be understood, however hard one try. If the Stanford-Binet I.Q. is objectionable as a comparative unit, the group test I.Q. is even more so. It is amazing that such a value should be advocated in a scientific treatise.

The fact that the writer of a popular treatise labors under the impulsion to reach definite conclusions for his readers may account for certain errors in fact, overstatements, and unjustified conclusions found in the present book. A few of these will be mentioned to substantiate the reviewer's notion that a book on race psychology which does not deal with the methods and results technically and critically cannot but falsify the status of existing knowledge and scientific opinion in the field. We are told on page 29 that "the work of Galton served to prepare the way for work more firmly grounded in experiment, and this is the work especially of Mendel who was able to discover three principles of heredity. . . ." Actually, Galton and Mendel were born in the same year (1822) and Mendel's *Versuche über Pflanzen-hybriden* was published in 1865, four years before Galton published *Hereditary Genius*. The fact that Mendel's work was not "rediscovered" until about 1900 does not validate Garth's statement, made in the easy, off-hand fashion of one set to write for the "general reader." On page 35 correlation coefficients between National Intelligence Test I.Q. and degree of white blood in Indians are presented as follows, by grades: 4th, 0.70; 5th, 0.76; 6th, 0.22; 7th, 0.23; 8th, 0.24. Garth's conclusion from these values is "that education tends to reduce to a minimum, or even nothing, the effect of the presence of Indian blood in the educated mixed-blood individual." The problems of genetics and of race psychology would indeed be simple if they could be solved in this easy fashion! As a matter of fact, these figures raise several questions without answering any at all. Among the questions suggested by the coefficients are those regarding the nature of the white "blood" in the several groups, the adequacy of the samplings, the reliability of estimates of degree of Indian blood, the testing methods, and the range of classes of "degree of Indian blood" used in calculating the correlation coefficients.

A gross interpretation of the work of Peterson and Lanier is made on page 84. Garth's statement is as follows: "Differences so far found in the intelligence of races can be easily explained by the influence of nurture and of selection. See again Peterson and Lanier. . . . The low I.Q. of Indians and Negroes are undoubtedly due to these factors." I do not know what Garth means by "selection" here, but as used by Peterson and Lanier the factor of selection and that of nurture are not similar in operation but are directly antithetical, as far as their interpretative significance goes. Peterson and Lanier found a decreasing race difference in comparing negroes and whites in Nashville, Chicago, and New York. They suggest as alternative explanations better environmental conditions or selective migration, with the further

statement that both may operate although their relative effects cannot be determined as yet. Garth adopts the first alternative and misunderstands the second to the extent of believing that it is evidence for the first.

A major defect of the book is the failure of the author to analyze critically the testing methods used to secure the data to which he refers or presents in such profusion. There is little discussion of the reliability or applicability of the various types of tests to the problems, and even less effort to find out whether or not the data mean what they are purported to mean. An example is the implicit acceptance of Klineberg's explanation of white superiority in test performance as due to superior "speed" where as in "accuracy" no race differences exist. Another illustration is found in the attitude towards Seashore's Tests of Musical Talent, group tests notorious for their low reliability. We are told that "but for the introduction of a clever and scientific set of group tests we should at this point be frustrated in any experimental evaluation of musical talent of races." Somewhat later the following *argumentum ad verecundiam* is noted: "We have given this description of the traits to be examined from the standpoint of the inventor of the tests, whose superior experience and extensive experimental studies give great weight to his observations." In general, one prefers to form an opinion as to the scientific merit of methods and results on the basis of their intrinsic value, as determined by accepted standards of evaluation obtaining within the given branch of science. Garth is too prone to accept methods and results at their face value.

Garth is "prejudiced" in favor of a nurtural explanation of observed race differences in performance. He deplores the fact that "stupid race prejudice" has operated to restrict the opportunities of "inferior peoples" and to influence opinion concerning the comparative abilities of different races. He closes the chapter on Intelligence and Euthenics with the hope that an educational engineer may arrive who will render to the Indian and to the Negro in America "the treatment consistent with their euthenic needs." While this may be an admirable sentiment it really has little to do with scientific race psychology. The "environmentalistic" uplift attitude which has influenced many psychologists to abandon the scientific point of view towards the problem of the causation of race differences in behavior is no more justified, in the absence of crucial evidence, than the implicit belief in heredity as an explanation of such differences. The fact that the latter hypothesis remains unproved does not establish the validity of the former. It is unfortunate that Professor Garth did not choose to remain unbiased on this question, since his arguments may give rise to widespread misapprehension on the part of uncritical readers.

Vanderbilt University LYLE H. LANIER

Psychology. By ROBERT S. WOODWORTH. Revised Ed. New York, Henry Holt & Co., 1929. Pp. xiv, 590.

Professor Woodworth has reorganized and completely rewritten this book—the first edition of which was extensively reviewed in this JOURNAL (Vol. 33, 1922, 430-435). The revision extends from the significant dropping of the subtitle ("A Study of Mental Life") and the rearrangement of the various chapters to modification of the style.

Woodworth's plan in the new arrangement of the material, which he states "is frankly an experiment" (iii), was "to start with the broader and less technical

topics and proceed to analysis and still more minute analysis" (19). In accordince with this plan he considers—after an introductory chapter in which he presents his idea of what psychology is and does—such general activities and factors as Intelligence, Memory and Learning, Heredity and Environment, and Action and Motivation. He then turns to analysis and takes up the special activities of Feeling and Emotion, Sensation, Observation, Thinking, and Imagination; and then he proceeds to "still more minute analysis" and treats of the physiological activities, viz., the activities of the endocrine glands and the nervous system, the latter of which "has most to do with integrating the various organs into a whole individual and giving shape to his activities" (501). The concluding chapter, Personality, is given over to the synthetic problem: to the treatment of the individual as a whole and of his social adjustments.

Despite drastic alterations of the book, its point of view remains unchanged. Notwithstanding the fact that Woodworth "professes to belong to no one of the schools" (20), he again gives us a functional psychology of the empirical and biological type that is cast in the stimulus-response mold. His "middle-of-the-roadness" consists in ignoring systematic questions; it does not mean that he writes without a point of view, or that he is catholic in his treatment of the subject-matter. His book is as biased in its direction as a polemical essay, but the beginning student is not made aware of it chiefly because controversy is taboo and few attempts are made at definition. The reader is allowed or forced, as in the first edition, to bring his own definitions to many of the technical terms employed in the book. Moreover, where definition is attempted, the results are common-sense and general, vague and uncertain. For example, his basic concepts, stimulus and response, are tautologically defined: "a stimulus is any form of energy acting upon a sense-organ and arousing some activity of the organism" (225); a response is "any activity of the organism that is aroused by a given stimulus." But that is not all; these concepts are no sooner presented than they are modified by the introduction of the organism as the mid-term in the S - O - R formula. The O is, like Freud's censor, the *deus ex machina;* it selects the stimulus, and determines the nature and extent of the activity aroused.

Woodworth, moreover, does not specify the kind of activities studied. Though psychology is said to be the scientific study of the activities of the individual as a whole (3), not all the activities of the whole organism are considered. The selection might be comprehensible if the phrase "individual as a whole" were unequivocally defined, but it is not; and the muster roll of the activities treated merely adds to our perplexity. That wholeness is not the criterion for inclusion, the reader very soon discovers. The activity of the circulatory system, for example, which extends throughout the entire body, is excluded; as is also—though with apologies (501)—the activity of the muscular system. On the other hand, such isolated activities as the knee jerk and the pupillary reflex are included. What is the principle of selection? Upon what basis is this activity included and that one excluded? The author does not state, and his exposition does not reveal. In desperation one is forced to take as a guiding principle the facetious definition that "psychology is what the psychologist considers."

The discredited law of exercise—particularly its negative aspect, atrophy through disuse—is introduced without defense or comment in an early chapter and is carried forward through the book as an explanatory principle. The Danish physiologist

Lange is said to be a psychologist (304).—But these are details (the like of which the informed reader can find many), that lie outside of the general scope of the present review.

The book is pleasantly and entertainingly written. The style, while still free and easy, and conversational, is more conventional than that of the first edition. The elimination of colloquialisms is a distinct gain.

The new arrangement, which breaks with tradition, is successful. The exposition proceeds from the general to the specific. Topics of immediate interest are first considered and then the exposition turns by gradual steps to topics more remote and abstract. Woodworth renounces the logical method of presentation for the psychological. The result is good—may more 'experiments' of this kind be made.

K. M. D.

Elements of Human Psychology. By HOWARD C. WARREN and LEONARD CARMICHAEL. New York, Houghton Mifflin Co., 1930. Pp. ix, 462. 118 illus.

In this revised edition of Warren's *Elements of Human Psychology* the authors attempt to recognize the contributions of the *Gestalt,* the introspective and the behavioristic methods to psychology. The general make-up of the book follows the older edition, though several changes have been made in chapter headings and the order of presentation. Considerable amplification and re-defining have been made which have served to increase the clarity and reading qualities of the material. Numerous pertinent illustrations are used with excellent effect, and should do what many elementary texts do not—make concrete the fundamental concepts of psychology.

Approximately one-third of the book is devoted to the nervous system and its functions. This seems justified by the many attempts of the authors to explain mental and behavioral functions by means of hypothetical functions of the nervous system. In an elementary text such explanations seem to lend themselves to unintentional explanatory principles, even though there are many cautions given the reader to recognize them as merely hypothetical.

The attempt to combine introspective and behavioristic methods has led to a direct comparison between them, since they are often used to explain the same facts. Whenever possible the association theory in terms of images is used; and the behavioristic (conditioned response) method, whenever the evidence for imagery is not conclusive. It is pointed out that motor imagery is difficult to classify since actually it may be "slight muscle responses." Verbal associations have this same difficulty. A 'word' may be analyzed psychologically as sound, visual, muscle, or other experience. The frequency of such difficulties as are found in "unconscious" associations leads one to the central cleavage between the introspectionists and the behaviorists. 'Human Psychology' is described as "the science which deals with the interaction between man and his environment by means of the nervous system and its terminal organs, together with the mental events which accompany this interplay." On reading the book one is inclined to favor the behavioristic viewpoint for two reasons: first, when the imagery-association theory does not seem adequate, the conditioned response theory does, and there is no evidence to support the possibility that it may *not* operate equally well where association between images is given as the explanation; secondly, because of the above, and because the "interaction between man and his environment" seems more important to the central

biological interests of most psychologies, one is inclined to accept the imagery as worthy of study, but inconsequential as a functional part of the interaction of man with his environment.

As in the older edition, the book contains an index and dictionary of psychological terms. An appendix of seven chapters includes the following: the central nervous system, neural and mental processes, the nature of color, tables, review questions, suggested class demonstrations, suggestions in using the book. The end of each main chapter contains references for subsidiary readings.

University of California JACK BUEL

Physique and Intellect. By DONALD G. PATERSON. New York, The Century Co., 1930. Pp. viii, 297.

In this clearly written and well-organized summary, Paterson contributes to the analysis of individual differences in "intelligence" and "temperament" by showing that they are almost totally unrelated to differences in physique or physical condition. The data from various experimental studies in this field are shown to be in remarkably close agreement, but their original interpretations have been quite diverse and often unwarranted by the evidence at hand. By inquiring as to the adequate control of such variables as age, sex, race, and social status, and limiting the interpretations of statistical results to those listed in texts on statistics, a very large proportion of the studies are found to be non-critical or at best only suggestive of a supposed relationship. "The belief that there is necessarily an intimate connection between physical and mental traits is 'easy to scotch but hard to kill.' The author believes that many of the cited investigations were obviously conducted for the purpose of collecting substantiating evidence rather than as impartial inquiries designed to ascertain the true status of the relationship between physique and intellect."

The content of the book can best be indicated by a quotation from the summary given in the final chapter.

"Our detailed survey of available quantitative evidence has demonstrated that prevalent notions regarding the intimacy of the relationship between physical traits and intellect have been greatly exaggerated. Search in the realm of gross anatomy for a physical correlate of intellect has yielded uniformly negative results. It appears that such structural characteristics as height and weight are correlated only slightly with intelligence, narrowly defined. Even measurements of head size and shape are found to be relatively independently variable with respect to intellect, and skeletal development measured by precise X-ray photography yields either zero or low correlations with intelligence. The same may be said of dentition. Physiological development, measured in terms of pubescense, is found to be relatively unrelated to mental development, and so are complicated morphological indices of body build.

"A more surprising finding is the negative trend of the evidence with respect to mental correlates of ordinary physical defects and certain presumably deleterious physical conditions. In addition, there is the disappointing outcome of glandular therapy as a predicted panacea for mental deficiency. The evidence seems quite clear that mental development proceeds in relative independence of physical condition except when disease processes or injuries directly attack the nervous system, especially the higher centers.

"The suggestion is frequently encountered that physical traits may be found associated to a greater extent with temperament than with intellect. Even here,

however, little optimism is justified. An intimate connection between body build and temperament has not been disclosed. The positive findings of Kretschmer, Wertheimer and Hesketh, Farr, and Mohr and Gundlach are restricted to definitely pathological material" (269 f.).

Having cleared the field of numerous hypotheses which were not adequately supported by experimental evidence, Paterson goes on further to suggest a number of experiments and theories which might logically follow this survey of the field. "Without doubt the negative trend of the evidence reviewed has important implications for theories of the nature and organization of personality. The findings clearly support the *theory of unique traits* as formulated by Thorndike, Kelley, Toops, Hull, and others" (p. 276). Paterson, furthermore, says that this theory was supported in the Minnesota Mechanical Ability investigation, with respect to I.Q., mechanical ability, and physical agility, which he holds to be unique traits each with respect to the other.

"Progress in personality evaluation and analysis will be furthered by the discovery of additional identifiable and measurable traits unique among themselves and unique with respect to the unitary traits already isolated and measured" (276 f).

The reviewer anticipates a difficulty in the theory of unique traits if it is extended to differences in sensory acuities and fine motor skills. Here the individual differences are so highly "unique" or specific for each performance measured that they lose nearly all value for the prediction of other performance, even those which would seem to be qualitatively related. We are greatly in need of another critical review similar to Paterson's which would cover theories which refer the bases of individual differences to *functional* constants, such as speeds of nervous conduction, and "nervous energy." It would not be at all unlikely to find that this set of theories which seem to be implicit in most of the literature on individual differences is resting on no firmer basis than the previously supposed relation between physique and intellect. Are we not neglecting the fact that the *same organism* can function in *many different ways*, and that these methods of work may be a basis of the individual differences?

The style and organization of the book afford a good example of scientific evaluation of hypotheses and their supporting evidence. The book is likely to find a useful place as a reference in courses and research bearing upon individual differences of either physique or intellect. It may be read profitably by teachers of elementary psychology since it shows up very clearly the defects of many popular ideas often considered in these courses. The students will not be the only ones to lose some cherished beliefs.

University of Oregon ROBERT H. SEASHORE

Psychology: Science or Superstition? By GRACE ADAMS. New York, Covici Friede, 1931. Pp. 299.

The criterion of the success of any discipline would seem to the author to be the amount of public interest in it; and this is apparently to be judged by the amount of drawing-room conversation and by the extent of newspaper publicity any discipline obtains. From this point of view a great deal of this volume might be summarized by the statement (p. 209) "until psychoanalysis captured the popular

imagination, there was no public interest in psychology." The corollary, which the author stresses, is the failure of experimental psychology. In this regard we find James in 1899 confessing: "In my humble opinion there *is* no 'new psychology' worthy of the name." To this statement, the author feels compelled to add (p. 269) "yet the science of psychology is essentially the same as it was thirty-two years ago. It has not made a single substantial or fundamental discovery since 1899."

The method of development of the book is historico-critical, beginning with Aristotle and ending with the present day. Miss Adams tries to show that few psychologists have completely divorced themselves from Aristotle's problem which was to "discover and ascertain the nature and essence of the soul." Indeed it is her opinion that William James and G. Stanley Hall are the only two American psychologists really worthy of consideration, primarily because both, in later years, denied the experimental method and developed their systematic positions along metaphysical lines. Of all the others, foreign as well as American psychologists, only Titchener and Watson (in his early period) stuck to their theoretical postulates with the result that their systems are sterile so far as popular interest is concerned and so far as they have had any effect on popular thought. This is because they were able to treat only of the simpler processes and found an experimental attack on the more complex processes impossible. All of the other psychologists, and many are mentioned and criticized, have based their psychological theories "not upon accumulated evidence but upon personal opinions" (p. 280). This group includes the psychoanalysts as well as the academic psychologists. So, with few exceptions, most psychologists "are still swimming, some floating, others with bold overhand strokes, in the soothing waters of metaphysics, which lead straight into the caverns of theology where man and God are fused."

The book is entertainingly written even though some points seem to be made largely by errors of statement or by ignoring certain materials. For example, mental tests gained their popularity under the application of Terman's "mathematical wizardry" to the Binet-Simon tests—a mathematical wizardry which involved the calculation of a simple ratio. Since the World War, which was the zenith of mental testing, "their only really noticeable expansion has been in the educational institutions where their inventors were teaching before they heard the call to military service." But what about all of the children in practically every school system in America where the giving of psychological tests are matters of routine? Again we find (p. 244) that when Köhler's apes found one stick too short to reach the bait, "he would snatch up two, hollow out the end of one, whittle away the end of the other, fit the two sticks tightly into the end of each other." But, unfortunately, Miss Adams does not tell us whether the ape went over and asked Köhler for the loan of his penknife or whether the ape made a knife for himself by chipping a flint on the iron hinge of his cage.

It is not worth while to unduly extend this review by citing all the errors of omission and commission. A few will suffice. Miss Adams has insisted that industrial applications of psychology have failed and that such applications have consisted merely in the giving of mental tests for the elimination of "inefficient employees in business" (p. 205). In contrast to this statement some space is given to an account of Anderson's work from a psychiatric approach to employment prob-

lems at Macy's store. No mention in this connection is made of the work of C. S. Myers, Viteles, W. V. Bingham or of any of the German workers in this field.

Apparently for Miss Adams, experimental work on industrial problems ended with the death of Münsterberg. She consequently proves her thesis—to her own satisfaction at least—that experimental psychology has failed and that psychoanalysis has captured the popular interest to the point that psychoanalysis *is* psychology outside of the academic institutions. Her statement is plain and unequivocal (pp. 117 ff.): "In other words, to the world outside the colleges, psychology now means neither empiric philosophy, nor mental physiology, but psychoanalytic hygiene." In support of this statement, the author (p. 178) lists 28 names of individuals who since 1909 have been awarded the "distinction" by the American Psychological Association "as its leaders." (Langfeld's name is spelled with a final "t.") The author asks the reader to examine this list and see how familiar the names may be. One wonders if the test would fare any better if applied to the names of the recent past-presidents and boards of governors of the Chemical, Physiological, Physical, or Biological Societies in America.

The author further says that "when a psychiatrist has exhausted the material obtained from patients and friends, he can always turn to literature" (p. 175). Following this suggestion, the reviewer recommends that Miss Adams obtain a copy of Aesop's fables and read the story of the fox without a tail.

University of Pennsylvania SAMUEL W. FERNBERGER

The Great Apes: A Study of Anthropoid Life. By ROBERT M. YERKES and ADA W. YERKES. New Haven, Yale University Press, 1929. Pp. xx, 652; 172 illus.

The Yerkes have brought together within the compass of this book all of the available information regarding anthropoid life. It is one of the most thorough and competently written hand-books of our time. It will long be used as a source and reference book by those interested in the great apes.

The work is divided into 46 chapters, arranged systematically into 6 parts. The 5 chapters of Part I are historical. Chapter 1 is concerned with ancient knowledge of anthropoid life; Chapter 2, with mediaeval knowledge; and Chapter 3 with 18th Century knowledge. Chapter 4 traces the emergence of our knowledge of the gorilla, an anthropoid unknown during the 18th Century; and Chapter 5 presents briefly the derivation of the popular and technical terms and the taxonomic and systematic information that is essential to an understanding and appreciation of the subject-matter. The four succeeding parts are devoted to the separate consideration of the different anthropoid types: Part II, the gibbon; Part III, the orang-outan; Part IV, the chimpanzee; and Part V, the gorilla. The materials in these different parts are, to the great convenience of the reader, arranged topically.

Part VI, composed of three chapters, supplies a comparative survey of the field. Chapter 44, the first of the three, gives a systematic comparison of the five principal anthropoid types—the siamang in addition to the four mentioned above. It closes with a résumé cast in tabular form (552-561). Chapter 45 compares similarly four of the representative types of primates; namely, lemur, monkey, ape, and man. It closes with a bibliography of 42 titles which relate specifically to the topics of the chapter. Chapter 46 gives a "summary of the anthropoid research in retrospect and prospect." The future seems bright.

The work closes with a comprehensive bibliography (593-617), and an excellent index (619-652). The bibliography "except for the omission of anonymous publications and articles in newspapers and popular magazines which seem to lack scientific value . . . is supposed to be complete through 1927" (footnote p. 593).

"Only incidentally does the volume support theses or present hypotheses; it is offered, in the disinterested spirit of science, to promote knowledge and enlightenment through the encouragement of honest, painstaking, unprejudiced observation" (ix). The authors have been eminently successful in their task.

K. M. D.

A Handbook of Statistical Nomographs, Tables, and Formulas. By J. W. DUNLAP and A. K. KURTZ. Yonkers-on-Hudson, World Book Co., 1932. Pp. vii, 163.

This book is included in the Measurement and Adjustment Series edited by Professor L. M. Terman, and seems to the reviewer to be the most useful single tool available to the worker in educational and psychological statistics. Having observed the manuscript in preparation, I am aware of some of the difficulties encountered, first in the preparation of the nomographs and tables and in the derivation of formulas, and second in securing the remarkable degree of accuracy attained. The authors believe that the error in reading the nomographs will not enter before the third or fourth significant figure of the final answer. I determined the standard errors of 100 means, first from Nomograph 5, and then with a calculating machine. These agreed in all instances to the second significant figure and in most cases to the third. Constants entering into construction of the tables have been taken to at least eight significant figures. All have been carefully checked and, in case of any doubt, recalculated. Many of the formulas have been independently derived by the authors and seem unusually free from error.

The usefulness of the nomographs (28 in number) in solving some 38 formulas is almost too patent to need comment. After using a few of them I should estimate a saving of from one-third to one-half the time required for the usual substitutions and calculations. The standardization of notation throughout the book is highly commendable. Several of the tables are new and will undoubtedly prove useful. Tables of probable errors are included, seemingly, for the sole benefit of those who have become accustomed to think in such terms rather than in terms of the standard error. The value of the book might be enhanced by a loose-leaf binding of some sort so that the nomographs, when being used, could be placed flat on a desk or table. Possibly they would be more durable if printed on a heavier and water-proofed paper. These are problems of mechanics, however, and may not be thought of as detracting seriously from the outstanding merit of the book.

Western Kentucky State Teachers College M. B. JENSEN

Die Seele und das Ich im Homerischen Epos, mit einem Anhang Vergleich mit dem Glauben der Primitiven. By JOACHIM BÖHME. Leipzig, Teubner, 1929. Pp. 130.

Böhme attempts to determine the order in which the different meanings belonging to a concept such as 'heart,' 'mind,' and 'self' have come into usage. He analyzes the text of Homer to ascertain the connotation of such terms as used by Homeric man. He also tries to determine the meaningful implications of such

terms in the mind of primitive man, relying chiefly in this case on the recent work of Arbman.

Böhme submits that there are three possible views in regard to the order of development of the meanings which a concept carries. First, there is the view that a word such as heart, which has both organic and mental or spiritual reference, had originally an organic meaning and gradually acquired broader shades of meaning including those of mental significance. Secondly, there is the opposite view that primitive man first used such words with mental signification and that organic reference had to await the accumulation of definite knowledge of anatomy and physiology. Thirdly, there is the view that primitive man used concepts in a generic sense embodying both organic and mental implications of meaning and that differentiation of the various shades of meaning had to await increase in knowledge in regard to mental as well as in regard to organic facts. Böhme subscribes to the third view on the ground that such words as hunger, fatigue, effort, and sex bear both organic and mental reference in Homeric writings, and also because concepts tend to have general, undifferentiated and non-specific meaningful reference in Homeric literature. Food revived Homer's soldier in spirit as well as in body. Even in the case of terms which are used today exclusively in a mental or spiritual sense, Böhme attempts to show that as used by Homeric man these words first carried a double meaningful connotation—bodily and mental—and referred to situations involving life processes or man's doings.

Although the author gives the impression of having been extremely careful in his analysis of meanings in the Homeric text and provides ample quotations to support his claims, the reviewer submits that at certain points any study of this type is bound to leave matters in a state of uncertainty and inconclusiveness because of insidious errors which attend subjective interpretation. The danger of attempting to press too many details under a general principle is perhaps not wholly escaped.

Wellesley College MICHAEL J. ZIGLER

The Meaning of Music. A Study in Psychological Aesthetics. By CARROLL C. PRATT. New York, McGraw-Hill Book Co., 1931. Pp. 253.

This is an attempt to achieve—through a melange of psychology (mainly revamped Titchener with a few side-glances at *Gestalt*) and of aesthetics—some ultimate knowledge of the way in which we perceive music and of how it affects us. Although not an impossible presentation of the problem, it approaches this more closely than would be expected from the author of so many excellent reports of experiments. The book has its good points, but its failings decidedly outnumber them. They arise principally from four factors: first, from attempting to thread the labyrinth of aesthetics without being thoroughly master of the field; secondly, from an apparent inability on the part of the author to crystalize a line of reasoning into definite tenets and then to keep them from coalescence; thirdly, from insufficient documentation—we had almost said knowledge—drawn from the psychological work done on the problem in Germany; and fourthly, from incoherent structure, due apparently to too hasty writing, and to the desire of the author to include in his text, no matter how irrelevant, the findings from his own experiments. Such a sweeping criticism calls in turn for much documentation in order to be fair to the author; but to criticize a book of aesthetics adequately, espe-

cially when there are so many points to be taken up, demands the writing of another book. Rather than involve ourselves in slippery and illusive arguments without sufficient space to define them, we refer anyone seeking knowledge on the meaning of music to Professor Pratt's book—with the single warning to expect no more enlightenment than he has previously obtained from earlier music aestheticians.

One point only must be mentioned. If we have been able to grasp the structure of the second section of the book correctly, the primary aim is to show that the chief problem in aesthetics would be solved if more stress were laid on "Movement in Music" (p. 185)—a factor "badly neglected in most treatises" (p. 110). Granted that no one so far (and this includes Pratt) has given an adequate psychological explanation or description of the phenomenon, the topic has certainly not been neglected. From the time of Aristoxenus' *Harmonics,* through Nichelmann and Hanslick, to Ernst Kurth's *Grundlagen des Linearen Kontrapunkts* (1916) and his *Musik-Psychologie* (1931), the topic has been discussed extensively. The first of Kurth's books mentioned opens with the statement: *"Melodie ist Bewegung."* E. M. von Hornbostel has also mentioned the idea in his *Psychologie der Gehörserscheinungen,* where he has briefly developed more of its implications than does Professor Pratt.

Cornell University R. S. HILL

Clasificación y estudio estadístico de 3,719 alumnos, de las escuelas secundarias de la cuidad de México por medio de exámenes mentales. By MONTANA LUCIA HASTINGS. Mexico City, Publications of the Secretary of Public Education, 1929. Pp. 192.

While this study is primarily intended to demonstrate to educators of Mexico the value of testing and the mental measurement movement in general as it may be applied to schools, it has a secondary importance—the scientific value of the data it contains. The author throughout the report is imbued with the idea of demonstrating to the reader the application of statistical method as illustrated by the results of her study. This probably accounts for the inclusion of elementary phases of statistical treatment and for the omission of facts valuable to the scientist. But as an educational document in its setting it has importance. It is probably the first and only study of its kind in Mexico.

The subjects of the study were not selected to be representative of all children of the ages considered, in the Republic of Mexico; but they represent the population to be found in the first year of secondary schools, particularly of the City of Mexico.

One of the tests used was a translated Army Beta test. The author had begun using the Beta test on Mexican children in the United States while writing a master's thesis at the University of California. She is an American who has transferred her activities, in the study of Mexican children, from the United States to the Republic of Mexico. The same methods and technique were used in Mexico as in the United States. The same testers, trained by the author, tested all the subjects throughout this study. The other test used was the Otis Test for Reasoning in Arithmetic, which the author says was easily translated into Spanish with no change or apparent loss to the test—excepting where she had to use the word "kilogram" for the word "mile." The work was done under the auspices of the

Director of Secondary Education and with the encouragement of Professor Moises Saenz, Sub-secretary of Public Education.

In addition to the children of the first year of secondary school, there were 145 of the sixth year of primary instruction, and 189 native Indians in a school called "The House of Native Students." These 334 were deducted from the 3,719 in the larger number. In the remaining group of 3385 there are 2567 boys, and 818 girls. The years of testing extended through the school years of 1926-28. The 1926 students were not tested with the Otis test.

The reviewer was able to obtain the following data by putting the results of smaller tables into larger ones: Score on Beta of 2567 boys (mean age 14.1 ± 1.1 yr.) is 70.7 ± 8.5; and that of 818 girls (mean age 14.07 ± 1.11 yr.) is 65.6 ± 9.3. Score for total 3385 (mean age 14.1 ± 1.1 yr.) is 69.3 ± 8.9. Score on Otis of 1782 boys is 11.4 ± 2.1, of 796 girls is 9.4 ± 1.9. Score for total 2578 is 10.8 ± 2.1.

University of Denver THOMAS R. GARTH

Die Kunst. By BRODER CHRISTIANSEN. Buchenbach, Felsen-Verlag, 1930. Pp. 260.

Christiansen is the author of at least two other books on problems in aesthetics, and apparently has won for himself a certain skill in the juggling of concepts and the construction of systems. The present essay stems from Goethe's mention of *"die sinnlich-sittliche Wirkung"* of colors and from one of Kandinsky's manifestos, from which several terms and fundamental concepts are borrowed. Christiansen maintains that an artistic object possesses two sides: the apparent outer "sensations" of colors, vowels, and tones; and their corresponding spiritual correlates or *Innenklänge*. Blue, for instance, not only exists as the sensation of blue, but also possesses the *Innenklang* of cold, reserve, and distance. Vowels range from soft and round to sharp and pointed. A circle is cold and reserved, and therefore has through its *Innenklang* more relation to blue, than the *Innenklang* of blue has to that of any other color. These elements, however, seldom exist alone, but usually in compounds of other elements. Just as vowels and consonants are linked together to form a "grammar of art," so their *Innenklänge* are joined through logical relations to form a grammar. According to Christiansen, aesthetics has previously dealt only with the outer grammar of artistic objects, and has therefore missed the true solution. He proceeds to elaborate by means of his new elements a theory of the tragic and comic and of structural unity.

In so far as Christiansen stresses that peculiar phenomenon of the interrelation of disparate sensory fields (bright tones and bright colors, sharp tastes and odors), already noted by Von Hornbostel and Köhler (*Gestalt Psychology,* 1929) and in so far as his *Innenklänge* offer an explanation thereof, his book makes a sound contribution to the discussion. But except for occasional illuminating sparks, his elaboration of the subject is usually too far-fetched, too unreal. For instance: all art flows from God. The artist is merely His instrument, creating without knowing what he creates. The work of art is His attempt to better and to render more logical creations which were not altogether successful on the first trial. Such phrases give the impression that Christiansen has not only woven enough rope with which to do away with himself, but also unfortunately with those few good ideas which the book contains.

Cornell University R. S. HILL

Studies in Philosophy and Psychology. By G. F. STOUT. London, Macmillan & Co., 1930. Pp. xiii, 408.

Save for one paper, these studies are corrected reprints of essays, reviews, and critical discussions covering the period from 1888 to 1927 and done in the best manner of British Empiricism. Of particular interest to psychologists are the papers on "The Herbartian Psychology" and "Ward as a Psychologist." The first is the only historical essay in the volume and brings freshly to notice the main features of the *Lehrbuch zur Psychologie* and the *Psychologie neu Gegründet auf Erfahrung, Metaphysik und Mathematik.* The second (Chapter V) is both a review of and a critical commentary upon Ward's *Psychological Principles.* Much of the criticism centers upon Ward's interpretation of genetic psychology, a criticism which earns a gracious rejoinder by Mrs. (Mary) Ward (pp. 123-127).

Chapters II, III, and IV deal, respectively, with voluntary action (prompted by Shand's discussion of "Attention and Will"), the perception of change and duration, and the nature of conation and mental activity. Already in these chapters empirical psychology and philosophy are brought together; and through the rest of the book philosophy dominates. The topics discussed are as follows: the common-sense conception of a material thing, things and sensations, the way in which memory-knowledge may be immediate, Bradley's theory of relations, Bradley's theory of judgment, Bradley's theory of truth and falsity, Russell's theory of judgment, the nature of error, immediacy, mediacy and coherence, real being and being for thought, some fundamental points in the theory of knowledge, and the nature of universals and proportions.

With respect to the paper on conation and mental activity Stout is now inclined to treat of self as an embodied self rather than as purely mental, an interpretation that might have been put on the original paper. He has now changed materially his views on things and sensations, remarking (i) that the external object is external to the embodied self, (ii) that matter and mind are ultimately and essentially distinct but ultimately and essentially inseparable, and (iii) that sensible appearance may be material but not physical. These changes in Stout's thought are given detailed expression in his recent Gifford Lectures. The paper entitled "Bradley on Truth and Falsity" adds to and partially corrects the views expressed in "Bradley's Theory of Relations." In so far as the article on "Error" disagrees with the paper on "Real Being and Being for Thought," Stout would choose the latter. A definitive definition and description of "presentations" is given in the paper on "Some Fundamental Points in the Theory of Knowledge."

University of Illinois COLEMAN R. GRIFFITH

The Nature of Life. By EUGENIO RIGNANO. New York, Harcourt, Brace & Co., 1930. Pp. x, 168.

The late Professor Rignano has here brought to systematic completion a point of view, various aspects of which were developed in his earlier works: "The Inheritance of Acquired Characters," "The Psychology of Reasoning," and "Biological Memory." The book presents a militant, at times almost an evangelical, exposition and defense of an energetic vitalism, a vitalism based not on a belief in entelechies or vital forces but on the assumption of a dynamic nervous energy which belongs within a causal and deterministic system. Such an hypothesis, the author holds, rep-

resents the only logical escape from the equally untenable extremes of pure mechanism and pure vitalism.

Rignano's views are too well known to require restatement, and this book contributes little that is new. He advances once more the hypothesis of mnemic traces, which renders possible a belief in the inheritability of acquired characters, and defends it as the only possible explanation of the purposive nature of all living phenomena. He develops somewhat further, too, the notion of fundamental affective tendencies, discussed at length in his *Psychology of Reasoning*, and shows how necessary such an assumption is to an understanding of social progress. The greater part of the book, however, is devoted to the defense of the thesis that all living activity, however simple, is essentially purposive. To this end he assembles great masses of facts and quotations, ranging in their reference from such elementary physiological phenomena as assimilation and metabolism to such social manifestations as justice and morality. It is an imposing array. But even to one who is in general sympathetic toward his point of view it seems regrettable that there is so little of argument and so much of invective. Even Loeb, the only mechanist who receives the courtesy of extended quotation, is chided for his prejudice rather than confronted with a reasoned argument. To the reviewer it seems that this will prove simply another book which will confirm the beliefs of those who agree and will be dismissed with a shrug by those who do not.

Cornell University R. B. MACLEOD

Geschehnis und Erlebnis. By ERWIN STRAUS. Berlin, Springer, 1930. Pp. vi, 129.

The book introduces itself with the promise to start from the "pension neurosis" and to end there and to sandwich in pictures of the "problem of time," which the author, who is a psychiatrist, declares to be "the central problem of theoretical psychology." There is nothing new, practical-psychiatrical, which the American psychologist could learn about the pension neurosis from this book. But he can learn from it how strong the tendency of contemporary German psychologists (and psychiatrists, it seems!) is to speculate philosophically rather than to solve the problems of psychical abnormality by biological methods. Psychoanalytic methods, which few American psychologists will admit to be strongly biological, are declared by the author to be altogether too strongly biological. Therefore he builds up his "psychology" on a philosophy of the "time experience." Essentially he means thereby the fact that a human being conceives his present life in relation to the past and the future, whereas an animal is not capable thereof. Thus he can speak of a "psychical trauma" or "deformation" easily in cases where there surely is no trauma in a neurological sense. This time-theory he believes is a contribution to a better understanding of the "pension neurosis" which has worried the German economists since sixty years ago and which since the creation of millions of world war veterans is worrying increasingly the economists of all the nations.

University of Missouri MAX F. MEYER

L'esprit et ses maladies. By MARCEL NATHAN. Paris, Rieder, 1930. Pp. 80, 60 illus.

Whoever is interested in the abnormalities of human behavior will find here much that is fascinating. Sixty pages are devoted to the reproduction of photographic studies which are quite remarkable. The reviewer cannot at this moment recall ever

having seen any collection quite so satisfying. Not only were the subjects well selected but all the techniques of photographic skill were drawn upon to make the effects as nearly life-like as possible. In some instances several photographs taken in quick succession are arranged in order upon the page, giving almost as good an idea of the action involved as would a cinematograph. And just about everything one could ask for is here, from reproductions of old drawings and artists' conceptions to encephalitic sequelae.

The text, of which there are but seventy pages, is much less attractive and important. It is merely a running description of the different forms of mental abnormality with a little about the ways of interpretation. The catholicity of mind and the scholarship revealed make pleasant reading. It is the sort of thing one would like to have available when one is asked by a person of intelligence and education for a brief reliable statement about the general field of psychopathology.

University of Oregon EDMUND S. CONKLIN

Philosophia Perennis. Abhandlungen zur ihrer Vergangenheit und Gegenwart. Festgabe Joseph Geyser zum 60 Geburtstag. Vol. I. Abhandlungen über die Geschichte der Philosophie. Vol. II. Abhandlungen zur systematischen Philosophie. Edited by FRITZ-JOACHIM VON RINTELEN. Regensburg, Josef Habbel, 1930. Pp. xviii, 525; x, 719.

The two volumes of this work consist of essays by representative Catholic scholars from all parts of the world (in German, French, Italian, Spanish, and English) which are published as a memorial of the sixtieth birthday anniversary of the Munich philosopher and psychologist, Joseph Geyser. America is represented by Professor Moorhouse F. X. Millar, of Fordham University with an essay entitled, "The Significance of St. Augustine's Criticism of Cicero's Definition of the State;" and England by M. C. D'Arcy and Leslie J. Walker, both of the University of Oxford with essays entitled respectively "Probability and Certainty," and "The Bearing of Modern Physical Theories on Catholic Philosophy."

In accord with the life-long attempt of Geyser to interpret the results of empirical science from the general platform of Aristotelian realism, the present work is divided into three parts, beginning with a sub-division devoted to problems in the history of philosophy, which is followed by a systematic part concerned with problems in the field of logic, epistemology, ontology, natural philosophy, psychology, ethics, and metaphysics. The third part of the collection consists of three essays, which attempt to evaluate the work of Geyser himself in the fields of psychology, epistemology, and metaphysics. Psychologists will be especially interested in the research and observations on emotions and sentiments by Agosto Gemelli, professor of experimental psychology at Milan, the psychology of thinking by Richard Hönigswald of the University of Freiburg, the psychology of value by Martin Honecker of the University of Breslau, and the discussion of Geyser's own contribution to psychology dictated by the late Max Ettlinger of the University of Münster from his deathbed.

Ettlinger calls attention to the fact that Geyser's philosophy and psychology developed through intercourse with the circle of students gathered at the University of Munich, for which Theodor Lipps, an "unusually stimulating teacher and personality" was the center. Lipps himself was struggling at the time with prob-

lems which were leading him away from his earlier psychologism in the direction of a metaphysics of mind which was especially congenial to Geyser. Geyser in his own development finds no conflict between empirical psychology of the type presented by Lipps and Brentano and a philosophical psychology, since, in his opinion, any psychological method, when carried out thoroughly, leads ultimately to metaphysics. In his appreciation of the experimental results of the Würzburg School, as well as in his utilization of the results of modern biological research, Geyser gives a concrete illustration of this doctrine. There is a brief discussion of Geyser's more recent contributions to the field of religious psychology which grew out of his criticisms and discussions of the work of Rudolf Otto and of Max Scheler. The two volumes are bound appropriately in scarlet cloth and are fine examples of the printer's art.

Vanderbilt University HERBERT SANBORN

Enriching the Curriculum for Gifted Children. By W. J. OSBURN and BEN J. ROHAN. New York, The Macmillan Co., 1931. Pp. xiv, 408.

The authors start with a clear, though perhaps somewhat exaggerated and not entirely unemotional, account of the harmful consequences which result in the ordinary school when all the children are given the same assignments and are expected to complete them within a given time regardless of their interests and abilities. The gifted child is described as possessing a large amount of energy, a strong emotional urge, and the ability to see likenesses and differences. The descriptions leave the impression that these qualities are entirely lacking in the dull child.

Considerable space is given to emphasizing the importance of taking into account the different types of ability in the selection of gifted children and that class standing is "very unreliable as an earmark of brightness." Yet in their own experiment the authors used class standing and the results of one reading test as the sole measures of the brightness of the child. The plan was to allow each pupil who was up in his school work to join a "hobby" club which met once a week for the study of some subject which was of special interest to him. The remainder of the pupils spent this period in remedial work. However, all but five or six pupils qualified for the "hobby" clubs before the end of the year, which leads one to question the appropriateness of the title.

Though the contrast between the theoretical and experimental sections of the book is disappointing, the reader will, nevertheless, find within its pages many constructive suggestions for meeting the individual needs and interests of his pupils.

Harvard University PSYCHE CATTELL

Foundations of Abnormal Psychology. By FRED A. MOSS and THELMA HUNT. New York, Prentice-Hall, 1932. Pp. xii, 548.

The authors distinguish two classes of mental disorders, the true ones and those which are pseudo-disorders. They mean by the former those which have a foundation in tissue pathology, by the latter those which are bad habits of behavior and can be removed by reëducation. They issue a warning against recognizing the latter as diseases (p. 72): "Man is by nature interested in the occult, . . . this was seen in the wide acceptance of the teachings of Mesmer and more recently in the case of Freud. . . . In Los Angeles at the present time we can demonstrate as grotesque

superstitious methods of dealing with mental disorders as have been so far reported in the history of psychology."

The book is divided into two parts, the first being a "general consideration" and the second a "consideration of specific diseases." The first part contains most of the historical matter, the discussion of "what is normal," and the general possibility of treatment. The second part considers the different kinds of bacterial infection, the different exogenous and endogenous toxins, glandular and nutritive disturbances, various pathological tissue abnormalities, the mental diseases of which the causes are virtually unknown, and those disorders which the psychoanalysts try to cure by their special methods.

Those who are still accustomed to find in a book on abnormal psychology chiefly discussions of the occult, the mysterious, the unconscious, will be disappointed, for none of those subjects are treated here, save to the extent which is necessary to give reasons for their omission. The book is intended for readers who accept the idea that scientific abnormal psychology and scientific psychiatry are essentially the same thing.

University of Missouri MAX F. MEYER

Children at the Crossroads. By AGNES E. BENEDICT. Drawings by Cyrus LeRoy Baldridge. Foreword by Howard W. Nudd. New York, The Commonwealth Fund, Division of Publications, 1930. Pp. 238.

This is a popular book, without references or index, intended to illustrate and emphasize by the use of appropriately selected cases certain general principles given in the summaries. The book is the first to present individual case-stories of maladjusted rural children treated by the visiting teachers in the program of the National Committee on Visiting Teachers. This Committee was created in about the year 1922 to administer for the Commonwealth Fund a program of thirty demonstrations of visiting teacher work. The two earlier books issued by the same fund, *The Problem Child in School,* by Mary B. Sayles, and *The Visiting Teacher at Work,* by Jane F. Culbert, deal with special aspects of the work of this Committee, of which Mr. Howard W. Nudd is chairman. The "cases" and drawings are interesting, and the book will doubtless increase the imagination and resourcefulness of teachers, parents, and social workers. J.P.

Magische Geisteshaltung im Kindesalter und ihre Bedeutung für die religiöse Entwicklung. By KARL ZEININGER. Beiheft 47 Zeitschrift für angewandte Psychologie; Beiträge zur Jugendpsychologie, No. 1. Leipzig, Johann Ambrosious Barth, 1929. Pp. x, 155.

The problem of the study is to trace the religious behavior of the individual from its early manifestation in the child to its adult form. The writer believes that in the child as in the race religious responses develop from the non-religious in the presence of the mysterious; early responses are classed as magic, taboo, sacrifice, and the like. The religious conceptions are drawn from such writers as Rudolph Otto, Karl Girgensohn, and Nathan Söderblom.

The monograph is stimulating in its attempt to cover scientifically certain aspects of life not ordinarily included in a treatise on human nature. The method of quoting incidents from a wide field illustrates but hardly proves what the writer has undertaken.

Florida State College for Women P. F. FINNER

The Activity Movement. By CLYDE HISSONG. Baltimore, Warwick & York, 1932. Pp. x, 122.

This dissertation is a critical appraisal of the psychological foundations of progressive education. Free activity as an instrument is accepted, but freedom as a goal is held to be an inadequate basis for education. On the other hand, Thorndike, who is regarded as typical of mechanistic and scientific psychology, is criticized for stressing unduly the possibility of deliberately controlling the learning process by manipulation of the environment, while neglecting the suggestion as to desirable goals which derive from a study of the developing potentialities of the young child.

Ohio State University — H. B. ENGLISH

The Principal Nervous Pathways. By ANDREW THEODORE RASMUSSEN. New York, The Macmillan Co., 1932. Pp. 73.

This atlas contains a valuable series of neurological charts and schemas, accompanied by concise explanations. Although designed primarily for use in courses in neuro-anatomy in the medical curriculum, psychologists and students of physiological psychology will find the book of great value. All of the sensory and motor systems, including the autonomic, are presented in great detail and with admirable clarity. The reviewer knows of no other atlas which contains such complete and convenient schematization of the nerve tracts.

Vanderbilt University — LYLE H. LANIER

Cosmic Consciousness: A Study in the Evolution of the Human Mind. By RICHARD MAURICE BUCKE. New York, E. P. Dutton & Co., 1931. Pp. xviii, 384.

This new (the seventh) edition should prove a source of considerable enjoyment to the devotée of solipsism. A notice is justified only from the slight danger that the title may lead the uncritical layman to judge from it that the book makes a contribution to psychology.

Western Kentucky State Teachers College — MILTON B. JENSEN

Social Psychology: The Psychology of Attraction and Repulsion. By JOHN JEFFREY SMITH. Boston, Richard G. Badger, 1930. Pp. xxv, 468.

This book is intended as a text for students of social psychology. Social psychology is considered as the study of social attraction and repulsion. Personal as well as group relations are considered. The book is a compendium of opinions culled from a host of authorities. These authorities are for the most part non-scientific and little space is devoted to the presentation of experimental results. Each chapter is followed by exercises. There is a bibliography of 252 titles and a complete index.

University of California — CALVIN HALL

Safety and Health in Organized Camps. By J. EDWARD SANDERS. New York, National Bureau of Casualty and Surety Underwriters, 1931. Pp. viii, 133.

This volume reports an investigation into the frequency and causes of accidents and illnesses in a large group of children's summer camps, and includes tentative recommendations as to administrative procedures. It bears only a slight relation to academic psychology.

Institute for Juvenile Research, Chicago — LUTON ACKERSON

Essays on Marriage. By FREDERICK M. HARRIS. New York, Association Press, 1931. Pp. vi, 208.

A collection of unfinished manuscripts posthumously edited by Harrison S. Elliott form the background for an orderly presentation of the primary problems of marriage. The breadth of analysis and depth of insight is based on broad reading and practical experience in counseling. A comprehensive survey of the present status of marriage and attitudes, customs and laws lead the writer to define marriage as a partnership. "The personal relationship involved in marriage is on the way to success when the depth and range of the interests shared between the two partners are sufficient to make the common enterprise significant and rewarding to both." This functional definition of marriage is discussed primarily about the crucial elements of "the strictly sexual relationships, the economic adjustment, and the children."

The most significant work is contained in the central chapters on "The Relation of Sexual Desire and Conjugal Affection," which are frankly discussed as entities—the former as an individual function and the latter as growing out of social experience in the family—and "the sexual relationships of marriage," which while based upon the sound basis of physiological function is understandable primarily in light of a breadth of sound knowledge on the part of the reader. It is this presumption which is apt to leave the average reader with little but a satisfactory two hours' reading in a field in which all are interested. From these central chapters on the discussion of the results of ignorance and silence the author goes on to a rather innocuous argument about selection of mates and the "limitation" of the family.

University of Pennsylvania R. A. BROTEMARKLE

Our Knowledge of Other Minds. By W. WYLIE SPENCER. New Haven, Yale University Press, 1930. Pp. 145.

A logical analysis is made of numerous mental characteristics usually taken as criteria of the existence of other minds. The following apparent evidences are examined reflectively before being rejected: the concept of a mind associated with a body; instinct; consciousness; sensory experience; mental process; emotions; and noetic powers. The argument from analogy of the existence of other minds is cast aside after a cursory treatment. The evidence most convincing to the author is the recognition of one's social conduct. We know other minds, it is claimed, in so far as we know that we are coöperating with other human beings in activity of such a nature as to call upon our full measure of conscious activity. "This explanation rests on the conviction of the existence of a social order in which one plays one's part among similars." Obviously, the above mentioned coöperation and conviction are the common possession of all; undoubtedly they support the argument from analogy of the reality of other minds. In general, the arguments are derived from the older concepts of psychology at the expense of the late experimental findings and the resulting concepts.

University of Utah M. C. BARLOW

Cours de Philosophie. I. Manuel de philosophie à l'usage des classes de Philosophie et de première supérieure. By A. CUVILLIER. Paris, Armand Colin, 1931. Pp. xxxiv, 722.

The volume is intended to serve as a general introduction to classes in philosophy and mathematics in the schools which prepare students for institutions of higher

learning in France, and is followed in the complete work by a second part, whose sub-title is *Logique, Morale, et Philosophie générale.* The author believes that through an introduction to the problems of philosophy by way of psychology the student will be furnished with a concrete foundation of material for later theoretical discussions which might otherwise degenerate into pure speculation. With this in mind a comprehensive survey of the general and special methods in use by psychologists today is given, hand in hand with a critical discussion of the data furnished by a large number of representative investigators in all fields. The American psychologists from whom material has been drawn are James, Jastrow, Titchener, Warren, Watson, and Yerkes.

Vanderbilt University HERBERT SANBORN

A Philosophy of Reality. By E. L. YOUNG. Manchester, Manchester University Press, 1930. Pp. xi, 266.

This book begins with the thesis that "things are what they seem." If ether vibrations are colored then a brain, in so far as it vibrates in the same way, is colored. So with all sensations. The objective world is made up of "marketable sensations" whereas a mind is erected upon a group of sensations that are no longer marketable because internal to the body. The essay then moves on through a study of spiritual life, the five senses, time, space and number, good and evil, and progress; but there is neither a continuous argument nor a suggestion of systematic thinking. A few names are mentioned in the text without further reference and there is no index. The author's philosophy of reality has a very unreal tang.

University of Illinois COLEMAN R. GRIFFITH

Marie Louise: Napoleon's Nemesis. By J. ALEXANDER MAHAN. New York, Thomas Y. Crowell Co., 1931. Pp. viii, 364.

The writer of this entertaining book attempts to show the influence upon Napoleon's downfall of Marie Louise's subconscious hatred of the French and Napoleon. He gives many references to documents and literature, but his psychology is naïve, expressing itself in terms of the prenatal influences which caused Marie Louise's attitudes and mentioning the various "parts" of her personality. At the same time, such well known factors as Napoleon's upbringing, his compensation mechanism for his small stature, and his physical disintegration are ignored.

Institute for Juvenile Research, Chicago LOWELL S. SELLING

Das Generationsproblem in der deutschen Dichtung der Gegenwart. By HEDWIG KOCH. Pädagogisches Mag., Heft 1308. Lagensalza, H. Beyer & Sons, 1930. Pp. 116.

The author believes that literature throws valuable light upon the problems arising out of the opposition of two generations. In contemporary German literature she finds the father-son conflict to be characterologically the most important and aesthetically the most impressive. The revolt of modern youth as expressed in literature shows itself in the attitude (1) toward members of the older generation individually, (2) toward the world of that generation with reference to (a) family, (b) school, (c) profession, (d) state.

The youthful generation is divided into four characterological types (1) protesting, (2) warring, (3) adapting, (4) resigning, and the influence of the *Zeitgeist* in the formation of these types is discussed. A bibliography is included.

University of Iowa BONNO TAPPER